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BY WILLIAM NICHOLSON.

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Of the Engravings the Subjects are, 1. Two Plates of Apparatus for Experiments on Heat; by Count Rumford. 2. A new

A new Electrical Instrument, by Mr. Wilson. 3. New Filtering Apparatus, by Professor Parrot. 4. Original Blow-pipe, by the Abbé Melograni. 5. Apparatus for Filtration, by Sir H. C. Englefield, Bart, &c. F. R. S. 6. Structure for purifying an entire Stream of Water. 7. Very simple striking Part of a Clock, by Mr. Prior. 8. An Instrument for counting the Number of Draughts from a Mine, by Mr. Antis. 9. An Instrument for delineating in Perspective. 10. Cheap Apparatus for drawing Ovals. 11. Sketch of Mr. Malton's Method of making very large Port-folios for Drawings and Prints. 12. A Lamp for burning Tallow, by Mr. J. W. Boswell. 13. An Evaporating Furnace, by Curadau. 14. A Gripe for the Safety of Carriages, by Mr. Bowler. 15. A perpetual First Mover. 16. A new Parallel Rule. 17. A new Compensation Pendulum, by Mr. Edward Troughton. 18. An Instrument for measuring the Absorption of Gases, by Morozzo. 19. Diagram to explain the Galvanic Energy upon Water, by C. Wilkinson, Esq. 20. Two Plates exhibiting a Condenser of Force, by which the Action of a variable First Mover is converted into a constant Pressure, by Prony.

Soho Square, December, 1804.

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SEPTEMBER, 1804.

ARTICLE I.

Letter from Mr. JOHN GOUGH, *containing a Narrative of some less common Effects of Lightning, by the Rev.* JONATHAN WILSON, *and Remarks by the Communicator.*

SIR, *Middleshaw, Aug.* 13, 1804.

THE following is a copy of a letter which I lately received from the Rev. Jonathan Wilson, Vicar of the parish of Biddulph, in Staffordshire. The facts and remarks contained in it, may prove acceptable to many of your philosophical readers, as well as to myself; and by giving it a place in your Journal, you will oblige

JOHN GOUGH.

Congleton, Aug. 1, 1804.

Stroke of lightning in a plain field, affecting a circle of 20 yards in diameter.

THE following circumstance induced me to defer my reply to your letter for a few days: On the 15th ult. I was informed in my way to church, by the farmer of Biddulph Hall, that, on the 7th, the lightning struck a plain arable field of his, which he uses at present for a cow pasture. I went with him, and found that the electric fluid had killed the tops of the taller thistles, in a circle of about twenty yards in diameter; but

the herbage and fmall thiftles, no higher than the grafs, did not appear to have been finged at all. Thofe in the centre, for about three yards, feemed much lefs injured than thofe nearer the circumference. In two places the foil was turned up, a few feet in length and an inch or two in depth; and where the ground ceafed to be broken up, one might perceive that the lightning had glanced, with feveral ramifications, along the furface, but under the long grafs, leaving a track, fuch as moles and mice fometimes make. In one place, where the impreffion made upon the foil was deepeft, and fomewhat refembled the letter V, a ftraight round hole appeared at the angle, which was about two feet deep, and about three inches in diameter.

Probability of a ftone having fallen from the atmofphere.

After I had left the place, it occurred to me that this hole might have been produced by a ftone falling from the clouds, a phenomenon which has lately caufed much fpeculation. The more I thought of this, the more probable it feemed. The hole was round and perpendicular, at no great diftance from the end of the barn, and a tall tree was alfo near the place; either of which was more likely to attract the lightning than the plain field. A very lofty tower, which was ftruck a few years ago, was about three times the diftance of the tree. The thunder was not fo loud as when the tower was ftruck, but the fmell of fulphur was much ftronger. Thefe confiderations determined me to return the firft opportunity and examine the place more thoroughly.

On examination none was found.

From one caufe or other I could not do this conveniently before yefterday evening, when the farmer and his fon very obligingly went with me, and dug a circular pit about four feet in diameter, having the hole in its centre. At the depth of two feet they came at a fhivery rock of grit, in which I hoped to find the expected ftone imbedded; but when the foil was cleared away level with the bottom of the hole, there was no fuch thing; nothing appeared but fmall, oblong, flat pieces of gritftone, through the moift crevices of which the electric fluid probably efcaped. Though difappointed in the principal part that I hoped to communicate, I refolved to fend you the above, as I think the lightning very feldom defcends fo far into folid ground, unlefs when guided by fome particular conductor. If the account afford you any information or amufement,

I fhall

I ſhall be glad; and ſhould the ſubject merit any of your queries, they will be received with pleaſure by

Your humble ſervant,

JONATHAN WILSON.

Remarks on the above.

THE firſt part of Mr. Wilſon's letter countenances Dr. Darwin's explanation of fairy-rings; for of all the thiſtles included in the electrified circle, thoſe in the centre received the leaſt injury. The hypotheſis of Dr. Darwin has always ſeemed to me to labour under unſurmountable difficulties; and this is the reaſon why I point out a fact which eſtabliſhes an exact agreement of the explanation, and natural appearances, in one inſtance. Whether fairy-rings be thus formed.

Fairy-rings are permanent objects, the nature of which is but imperfectly underſtood; and it is to be wiſhed that Mr. Wilſon would find leiſure to obſerve and deſcribe the future conſequences of the electric diſcharge at Biddulph Hall. The herbage of the ramified figure mentioned in the letter, will in all likelihood die in the courſe of a few weeks; becauſe plants that have ſuſtained ſtrong electrical ſhocks, ſeldom ſurvive the operation more than a month or two. Should this change take place in the circle, it will give a new appearance to the ſurface of the ground, by expoſing to view the branched path of the lightning; nor is it improbable but that this alteration will be ſucceeded by another of a more ſingular kind. There is reaſon to ſuſpect, that each ramification of the track will be again covered with a lively verdure the next ſpring, being accompanied at the ſame time by a contiguous patch of blaſted herbage reſembling its own figure. This ſuſpicion is countenanced by certain obſervations of Dr. Hutton of Edinburgh, who remarked, that the fairy-rings upon Arthur's Seat annually increaſe in diameter; *i. e.* the withered circumference of each circle becomes green in ſpring, and is ſurrounded in a ſhort time by a freſh ring of a ruſſet colour. We have no right to diſpute the juſtice of Dr. Hutton's obſervations, but the queſtion is undetermined, whether the appearance is conſtant, or reſults from the nature of the ſoil. The peruſal of the foregoing remarks will perhaps admoniſh my friend Mr. Wilſon, that Fortune has furniſhed him with an opportunity Compariſon with Dr. Hutton's obſervations of the fairy-rings on Arthur's Seat.

of profecuting the inquiry, fuch as fhe rarely affords to perfons of correct obfervation. I will not trefpafs upon your pages by a comment on the remaining facts of my friend's letter; let them fpeak for themfelves; in particular, the preference of a thunder cloud to low ground, in the prefence of lofty objects, including a tall tree.

JOHN GOUGH.

ANNOTATION.

I BELIEVE the opinion that fairy-rings are caufed by lightning, is of confiderable antiquity. Dr. Prieftley gave fupport to this conclufion by his experiment of the concentric rings formed on a polifhed metallic furface by the explofion of a battery. An effect of this kind produced in Kenfington Gardens, is defcribed, with an engraving, in our Quarto Series, Vol. I. p. 546.

W. N.

II.

On the Computation of Tables of Squares and Cubes. In a Letter from E. O.

To Mr. NICHOLSON.

SIR,

Computation of fquares and c ubes.

I SHOULD not have troubled you with any remarks on the method of computing fquares and cubes, if I had not feen the paper which was printed on that fubject in your Journal for laft July. But as it appears, from that letter, that there may be perfons who would like to employ themfelves on thefe calculations, it is defirable that the plaineft and eafieft methods fhould be pointed out to them.

Every one who is accuftomed to calculation, is acquainted with the advantage of conftantly repeating the fame operations. When the refult of any calculation is to be made out, in fome parts by multiplication and in others by divifion, fubtraction, or addition, it is impoffible for the moft unwearied diligence to avoid occafional miftakes. It is right, therefore, to fimplify as much as poffible in this refpect; and if the calculation

Computation of squares and cubes.

lation cannot be effected by a single operation, I believe I shall be joined by all persons accustomed to arithmetical computation, in recommending the use of addition * in preference to subtraction, and of multiplication in preference to division. Your correspondent H. G. does not seem to have paid sufficient attention to these considerations; and I fear that he not only would be soon wearied, if he were to work by the method which he recommends, but (what is of still more consequence) he could not depend for any continuance upon the accuracy of his computations.

The only method by which an extensive table like Mr. Councer's could have been calculated, must have been by the constant addition of differences; and the rules for this method may be easily deduced from the following considerations: Let $x+3a$, $x+2a$, $x+a$, &c. be any numbers in arithmetical progression. Then by the binomial theorem,

	1st Differences.	*2d Differences.*
$\overline{x+3a}\rvert^2 = x^2 + 6ax + 9a^2$		
	$2ax + 5a^2$	
$\overline{x+2a}\rvert^2 = x^2 + 4ax + 4a^2$		$2a^2$
	$2ax + 3a^2$	
$\overline{x+a}\rvert^2 = x^2 + 2ax + a^2$		$2a^2$
	$2ax + a^2$	
$x^2 = x^2$		

It will be foreign from my present purpose to enter into a particular consideration of the manner in which these differences arise. It will be sufficient to remark, that the second difference is constant, and that its value will be always 2, when we consider a as $=1$, or the progression $x+3a$, $x+2a$, &c. as a series of common numbers differing from one another by unity. Hence the first differences of the squares of such numbers become themselves an arithmetical progression, of which the common difference is 2; and consequently, if we know the first difference between the squares of any two such numbers, we can find the difference between the squares of the two next.

* If this opinion wanted any argument in its favour, I might support it by the authority of that able mathematician Captain Mendoza, who has calculated his new tables in such a manner as to make all the equations additional.

What

Computation of ſquares and cubes.

What has been ſaid may be exemplified by applying it to the continuation of Mr. Councer's ſquares. For

798684121 is the ſquare of 28261,
798627600 is the ſquare of 28260,

56521 is their difference, therefore 56523 is the difference between the ſquare of 28261 and the ſquare of 28262. By the addition in every inſtance of 2, we may find the ſeries of differences; and by the conſtant addition of them, we may eaſily find the ſquares. Thus

$$\begin{array}{r}
798684121 = \overline{28261}|^2 \\
56523 \\
\hline
798740644 = \overline{28262}|^2 \\
56525 \\
\hline
798797169 = \overline{28263}|^2 \\
56527 \\
\hline
798853696 = \overline{28264}|^2 \\
56529 \\
\hline
798910225 = \overline{28265}|^2
\end{array}$$

By a ſimilar method the table of cubes might be continued by the addition of differences; but, in this caſe, the calculation will be a little more complicated: Becauſe, if the terms of any arithmetical progreſſion be raiſed to the nth power, there will be n orders of differences, and the laſt will be the only one of them which will be conſtant. For the cube, therefore, where $n = 3$, we ſhall have one order of differences more than for the ſquare.

As before, let the progreſſion be $x + 3a$, $x + 2a$, $x + a$, x, &c.

Computation of squares and cubes.

	1ſt Difference.	2d Difference.	3d Diff.
$\overline{x+4a}\rvert^3 = x^3 + 12ax^2 + 48a^2x + 64a^3$	$3ax^2 + 21a^2x + 37a^3$		
$\overline{x+3a}\rvert^3 = x^3 + 9ax^2 + 27a^2x + 27a^3$	$3ax^2 + 15a^2x + 19a^3$	$6a^2x + 18a^3$	$6a^3$
$\overline{x+2a}\rvert^3 = x^3 + 6ax^2 + 12a^2x + 8a^3$	$3ax^2 + 9a^2x + 7a^3$	$6a^2x + 12a^3$	$6a^3$
$\overline{x+a}\rvert^3 = x^3 + 3ax^2 + 3a^2x + a^3$	$3ax^2 + 3a^2x + a^3$	$6a^2x + 6a^3$	
$x^3 = x^3$			

If therefore we make $a = 1$, the third difference will be conſtantly 6, and the ſecond difference will become $6x + 18$, $6x + 12$, $6x + 6$; ſo that if any three numbers be taken in an arithmetical ſeries (in which $a = 1$), the ſecond difference of their cubes will be conſtantly ſix times the middle number.

For the ſake of example, we may apply theſe obſervations to the continuation of Mr. Councer's cubes. We may conſider 26559, 26560, 26561, as part of an arithmetical ſeries: by the table we could find the difference of the cubes of 26559 and

Computation of ſquares and cubes.

and 26560, and, by what has been obſerved above, the ſecond difference of the three cubes will be* $6.26560 = 159360$; which, added to the difference between the cubes of 26559 and 26560, will give the difference between the cubes of 26560 and 26561. So far we ſhould proceed exactly in the ſteps which are traced out by your correſpondent H. G.; but I differ from him in this reſpect, that I ſhould by no means recommend this method for the ſolitary calculation of any particular cube: it is only mentioned here for the ſake of ſhewing how to aſcertain the numbers by which we muſt commence the ſeries of differences, when we want to continue a table which has already been calculated to a certain extent. For the third difference is conſtantly 6; therefore if we take an arithmetical progreſſion, of which the terms differ from one another by that number, and which begins with 159360 (ſuch as 159360, 159366, 159372, &c. &c.), we ſhall aſcertain the ſecond differences of the cubes which we want to find. Theſe ſecond differences muſt be added to the reſpective firſt differences, and, by that means, we ſhall find a ſeries of numbers, which, added to each particular cube, will give us the cubes of the numbers next above it. Thus

$$\overline{26560}|^3 = 18736316416000$$
$$\overline{26559}|^3 = 18734200194879$$

2116221121	=	$\overline{26560}\|^3 - \overline{26559}\|^3$
159360	2d Difference.	
2116380481	=	$\overline{26561}\|^3 - \overline{26560}\|^3$
159366	2d Difference.	
2116539847	=	$\overline{26562}\|^3 - \overline{26561}\|^3$
159372	2d D.	
2116699219	=	$\overline{26563}\|^3 - \overline{26562}\|^3$
159378	2d D.	
2116858597	=	$\overline{26564}\|^3 - \overline{26563}\|^3$
159384	2d D.	
2117017981	=	$\overline{26565}\|^3 - \overline{26564}\|^3$

Having thus aſcertained the firſt differences, we may proceed to add them to the cubes:

* The dot (.) between the figures is here uſed as the ſign of multiplication. N.

Computation of ſquares and cubes.

18736316416000 $= \overline{26560}\rvert^{3}$
2116380481 1ſt Diff.

18738432796481 $= \overline{26561}\rvert^{3}$
2116539847 1ſt D.

18740549336328 $= \overline{26562}\rvert^{3}$
2116699219 1ſt D.

18742666035547 $= \overline{26563}\rvert^{3}$
2116858597 1ſt D.

18744782894144 $= \overline{26564}\rvert^{3}$
2117017981 1ſt D.

18746899912125 $= \overline{26565}\rvert^{3}$

The methods here ſtated are, probably, the eaſieſt which can be deviſed for conſtructing an extenſive table: but it muſt frequently happen, that the calculator will want the ſquare or cube of ſome number which is greater than any which is contained in the table. It may be uſeful, therefore, to conſider the aſſiſtance which the table may afford him in facilitating the computation.

It is well known, that $x^m \times y^m = \overline{xy}\rvert^m$. * Therefore, if we want to find the ſquare of a number which is *e* double of any contained in the table, we have only to multiply the given ſquare by 4. In the ſame manner, if we want to find the ſquare of a number which is exactly three or four times as great as any contained in the table, we may find it by multiplying the given ſquare by 9 or 16. Thus, for example,

16522531600 = the ſquare of 128540;
4

66090126400 = the ſquare of 257080;

16522274521 = the ſquare of 128539;
9

148700470689 = the ſquare of 385617.

* I do not know whether it is worth while to mention the circumſtance, but your correſpondent H. G. has made a miſtake in the application of this rule. For he ſays that, if we "multiply the cube of any given root by 8, the product will be the cube of twice the next root:" whereas the product will be the cube of twice the given root.

In

Computation of ſquares and cubes.

In this manner we may find the ſquares of all even numbers which do not exceed the double of the table: we may alſo find the ſquares of numbers, to a greater extent, which are multiples of 3: the multiples of 4 and 5 may be ſquared likewiſe, if we multiply by 16 and 25; but ſquare numbers increaſe very rapidly, and conſequently this method can be only uſed (with advantage) for the ſmall multiples of the tabular numbers. We muſt, therefore, conſider ſome other method which may be applicable to prime numbers and multiples of large ones.

The ſquare of $2x+1 = 4x^2+4x+1 = 2x^2+4x+2+2x^2-1 = 2.\overline{x+1}|^3 + 2x^2 - 1$, or $2.\overline{\overline{x+1}|^3 + x^2} - 1$. If, therefore, we want to find the ſquare of an odd number which does not exceed the double of thoſe which are ſquared in the table, we muſt divide the next even number below it by 2, and this half will be equivalent to x in the above equation; the next number above this half will give us $x+1$. Having theſe two ſmaller numbers, we may find their ſquares by the table; and the ſum of their ſquares multiplied by 2, will exceed the ſquare required by 1. Thus, if it were required to find the ſquare of $257079 = 2.128539 + 1$: Here $x = 128539$, and $x+1 = 128540$; therefore, by Mr. Councer's table, the ſquare of 128540 would be 16522531600

the ſquare of 128539 would be 16522274521

33044806121

2

66089612242

Therefore the ſquare of 257079 is 66089612241.

Inſtead of examining each particular caſe, when the number to be ſquared is more than the double of thoſe in the table, it will be beſt to conſider the theorem, from which a general rule may be deduced. $\overline{nx+a}|^2 = n^2x^2 + 2anx + a^2 = nx^2 + 2anx + na^2 + \overline{n^2-n}.x^2 + \overline{1-n}.a^2 = n.\overline{x+a}|^2 + n.\overline{n-1}.x^2 + \overline{1-n}.a^2 = n.\overline{x+a}|^2 + \overline{n-1}.x^2 + \overline{1-n}.a^2$. Hence we muſt multiply x^2 by $\overline{n-1}$, to the product add $\overline{x+a}|^2$, multiply the ſum of theſe two quantities by n, and this laſt product will exceed the ſquare required by $\overline{n-1}.a^2$. The only caution neceſſary, is in dividing the given number by x; for as we

we ſuppoſe it equal to $nx + a$, we ſhould make x as great as it poſſibly can be, without being ſo large, that when added to the remainder a (as $x + a$), it ſhould exceed the numbers of which the ſquares are given in the table. Becauſe n and a are the only numbers by which we have to multiply, and as a muſt be leſs than n, we ſhall always have them as ſmall as poſſible, if we take x according to the directions here given. If, for example, it be required to find the ſquare of 385618, which is greater than twice, and leſs than three times 128540 (the greateſt number which is ſquared by Mr. Councer's table); here $385618 = 3.128539 + 1$; therefore if $n = 3$, $x = 128539$, and $a = 1$, $x + a$ will be $= 128540$, and will be within the extent of the table: Hence

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x^2, or the ſquare of 128539, would be by the table 16522274521

$n - 1$, or $3 - 1$ - - 2

33044549042

$\overline{x + a}|^2$, or the ſquare of 128540, would be 16522531600

49567080642

n or 3 - - 3

148701241926

and as $\overline{n - 1}.a^2 = 2$, the ſquare of 385618 will be 148701241924

If the ſquare required were that of 385619, in this caſe $a = 2$, and conſequently, if $x = 128539$, $x + a$, or 128541, would exceed the extent of the table: therefore n muſt, in this caſe, be equal to 4, $x = 96404$, and $a = 3$, and

the ſquare of 96404 = 9293731216

3

27881193648

the ſquare of 96407 = 9294309649

37175503297

4

148702013188; but $\overline{n - 1}.a^2 =$

3.9, or 27; therefore the ſquare of 385619 is } 148702013161.

From this example we ſee the neceſſity of being careful that $x + a$ does not exceed the numbers in the table: at the ſame

Computation of ſquares and cubes.

ſame time it may be remarked, that this is an extreme caſe, which can never occur but under particular circumſtances: for it only happens when the number to be ſquared is leſs than a multiple of the higheſt number in the table, and greater than the ſame multiple of the number next leſs than the higheſt; that is, for a table like Mr. Councer's, it muſt be leſs than $m.128540$, and greater than $m.128539$: under theſe circumſtances, if a be greater than 1, the caſe will occur which is the ſubject of the above caution.

It remains for us to conſider the method of finding the cubes of numbers which exceed thoſe given in the table.

Upon the principle to which we referred before, that $x^m \times y^m = \overline{xy}|^m$, we may find the cube of a number which is a multiple of any one contained in the table, by ſimple multiplication: for the cube of any number multiplied by 8, will give the cube of double that number; the cube multiplied by 29, 64, or 125, will give us the cube of 3, 4, or 5 times the number: Thus,

$$\begin{array}{rl}
3368928644271 & = \text{the cube of } 14991 \\
8 & \\
\hline
26951429154168 & = \text{the cube of } 29982. \\
 & \\
1045678375000 & = \text{the cube of } 10150 \\
27 & \\
\hline
7319748625000 & \\
2091356750000 & \\
\hline
28233316125000 & = \text{the cube of } 30450.
\end{array}$$

But as the cubes increaſe more rapidly even than the ſquares, it will ſtill be more neceſſary in this caſe than in the former, to eſtabliſh ſome means of finding the cubes of thoſe high numbers which are either prime or not exactly a ſmall multiple of a number contained in the table; and by proceeding in a manner ſimilar to that which we uſed for the ſquares, we eaſily eſtabliſh a general rule for this purpoſe: For $\overline{nx+a}|^3 = n^3x^3 + 3an^2x^2 + 3a^2nx + a^3 = nx^3 + 3nax^2 + 3na^2x + na^3 + \overline{n^3-n}.x^3 + \overline{3an^2-3an}.x^2 + a^3 - na^3 = n.\overline{x+a}|^3 + n.\overline{n^2-1}.x^3 + n.\overline{n-1}.3ax^2 + \overline{1-n}.a^3 = n \times \overline{\overline{x+a}|^3 + \overline{n-1}.\overline{n+1}\,x^3 + 3ax^2}| + \overline{1-n}.a^3$. Determine, therefore, n, x, and a, in the ſame

manner

manner as for the ſquare, and be equally careful that $x+a$ does not exceed the numbers in the table; add $\overline{n+1}\,.\,x^3$ to $3ax^2$; multiply the ſum of theſe two quantities by $n-1$; to the product add $\overline{x+a})^3$, and multiply this ſum by n: this laſt product will exceed the cube required by $\overline{n-1}\,.\,a^3$.

Computation of ſquares and cubes.

If, for example, it be required to find the cube of 53119, or $2.26559+1$: Then $n=2$, $a=1$, $x=26559$, and $x+a=26560$, and the cube of $53119 = 2\times \overline{\overline{26560})^3 + 3.\overline{26559})^3 + 3.\overline{26559})^2} - 1$: Therefore, by Mr. Councer's table, the cube of 26559 would be 18734200194879

The ſquare of 26559 -	705380481
	18734905575360
	3
	56204716726080
The cube of 26560 -	18736316416000
	74941033142080
	2
	149882066284160
therefore the cube of 53119 is -	149882066284159

Laſtly, let it be required to find the cube of 79601, or $3.26533+2$: here $n=3$, $a=2$, $x=26533$, $x+a=26535$; therefore, by what has been demonſtrated, $\overline{79601})^3 = 3\times\overline{\overline{26535})^3 + 2\times 4.\overline{26533})^3 + 6.\overline{26533})^3} - 2.8$.

$\overline{26533})^2 = 704000089$	$\overline{26533})^3 = 18679234361437$
3	2
2112000267	37358468722874
	2112000267
	37360580723141
	4
	149442322892564
	$\overline{26535})^3 = 18683458680375$
	168125781572939
	3
	504377344718817
Therefore the cube of 79601 is -	504377344718801

E. O.

Analyſis

III.

Analysis of a Triple Sulphuret of Lead, Antimony, and Copper, from Cornwall. By CHARLES HATCHET, *Esq. F. R. S. From the Philosophical Transactions for* 1804.

History of the mineral.

THE fubftance which forms the fubject of this paper, has hitherto been regarded as an ore of antimony; it is extremely rare, and has only been obtained from Huel Boys, in the parifh of Endellion, a mine which, from deficiency of profit, has for fome time been abandoned.

The fcarcity of the ore has probably been the caufe of its being unknown to foreign mineralogifts; indeed few even of the Britifh cabinets poffefs it; but the moft perfect and beautiful fpecimens are (as far as I know) to be feen in the fplendid collection of Philip Rafhleigh, Efq. of Menabilly, in Cornwall.

To Mr. Rafhleigh we are indebted for the firft defcription of this ore *; but no fubfequent notice had been taken of it, until the preceding paper was written by the Count de Bournon, whofe eminent merits, as a mineralogift and cryftallographer, are well known to this Society.

I.

Sp. gravity.

The fpecific gravity of this fubftance is 5766, 65° of Fahrenheit.

II.

Heat by the blow-pipe expelled fulphur and white fumes, and left fulphuret of lead and metallic copper.

If fuddenly heated on charcoal, by the blowpipe, it crackles and fplits; but, when gradually expofed to the flame, it liquifies, and, upon cooling, affumes a dull metallic grey colour.

When the globule was longer expofed to heat, white fumes (which at firft had a fulphureous odour) were evolved, and partly fettled on the charcoal.

Ebullition prevailed during the difcharge of thefe white fumes; and the globule gradually fuffered confiderable diminution, remaining at length tranquil, and of a very dark gray colour.

* Specimens of Britifh Minerals, felected from the Cabinet of Philip Rafhleigh, Efq. F. R. S. &c. Part I. page 34, Plate XIX.

Upon

Upon examination, this appeared to be principally ſulphuret of lead, which, like a cruſt, enveloped a minute globule of metallic copper, ſo malleable as to bear to be flattened by a hammer.

III.

Some of the ore, finely powdered, was put into a matraſs, and nitric acid diluted with an equal portion of water was poured on it. Upon being digeſted in a low heat, a conſiderable part, was diſſolved, with much effervеſcence. Some ſulphur, which floated, was ſeparated; and the clear liquor, which was bluiſh green, was decanted from the reſiduum at the bottom of the veſſel.

Digeſtion with dilute nitrous acid, &c. indicated ſulphur, lead, copper, and antimony, with a little iron.

A great part of the exceſs of acid being expelled from the ſolution, it was largely diluted with diſtilled water, and ſome diſſolved muriate of ſoda was added; but this did not produce any alteration in the tranſparency of the liquor. A ſolution of ſulphate of ſoda was then poured in, and formed a very copious precipitate of ſulphate of lead.

When this had been ſeparated, the liquor was ſaturated with ammonia; by which it was changed to a deep blue colour. A few flocculi of iron were ſeparated; and the remainder was found to contain nothing but copper.

The ſulphur which had floated, was added to the reſiduum which had ſubſided to the bottom of the matraſs; and the whole was digeſted with muriatic acid. This ſolution was of a ſtraw colour; and, when ſeparated from the ſulphur, and poured into a large quantity of water, afforded a plentiful white precipitate.

This precipitate was completely reſolved into white fumes, by the blowpipe; and the muriatic ſolution of it, when added to water impregnated with hydro-ſulphuret of ammonia, formed the orange coloured precipitate, commonly known by the appellation of golden ſulphur of antimony.

IV.

Muriatic acid did not immediately act upon the pulverized ore; but a ſolution was ſpeedily effected by the addition of a few drops of nitric acid: pure ſulphur was ſeparated; and the liquor, being decanted into water, yielded a copious precipitate of oxide of antimony.

The ſame reſults by treatment with muriatic acid and a little nitric.

 The

The filtrated ſolution, by gradual evaporation, afforded cryſtals of muriate of lead; and the lead which afterwards remained in the liquor, was ſeparated by a few drops of ſulphuric acid.

The ſolution was now of a bright green colour, and, as before, was found only to contain copper, and a minute portion of iron; the former was therefore precipitated in the metallic ſtate, by a plate of zinc.

Theſe experiments, with others which I have not thought neceſſary to mention, prove, that the conſtituent parts of this ore are lead, antimony, copper, and a little iron, combined with ſulphur; and, when the ſpecific gravity, the external and internal colour, fracture, grain, and other characters are conſidered, there can be no doubt but that at leaſt the three firſt metals exiſt in the ore, in, or nearly in, the metallic ſtate, combined with ſulphur, ſo as to form a triple ſulphuret; to aſcertain the proportions of which, the following analyſis was made.

V. *Analyſis.*

Analyſis. Pulverizing.

A. 200 grains of the ore, reduced to a fine powder, were put into a glaſs matraſs, and, two ounces of muriatic acid being added, the veſſel was placed in a ſand-bath. As this acid, even when heated, ſcarcely produced any effect, ſome nitric acid was gradually added, by drops, until a moderate efferveſcence began to appear.

Digeſtion with nitro-mur. acid,

The whole was then digeſted in a gentle heat, during one hour; and a green coloured ſolution was formed, whilſt a quantity of ſulphur floated on the ſurface, which was collected, and was again digeſted in another veſſel, with half an ounce of muriatic acid.

left *ſulphur*.

The ſulphur then appeared to be pure, and, being well waſhed and dried on bibulous paper, weighed 34 grains: it was afterwards burned in a porcelain cup, without leaving any other reſidium than a ſlight dark ſtain.

The green ſolution afforded, by dilution, *oxide of antimony*,

B. The green ſolution, by cooling, had depoſited a white ſaline ſediment; but this diſappeared upon the application of heat, and the addition of the muriatic acid in which the ſulphur had been digeſted.

The ſolution was perfectly tranſparent, and of a yellowiſh green: it was made to boil, and in this ſtate was added to three

quarts

quarts of boiling diftilled water, which immediately became like milk; this was poured on a very bibulous filter, fo that the liquor paffed through before it had time to cool; and the white precipitate thus collected, being well edulcorated with boiling water, and dried on a fand-bath, weighed 63 grains.

C. The wafhings were added to the filtrated liquor; and the whole was gradually evaporated at different times, between each of which it was fuffered to cool, and remain undifturbed during feveral hours. A quantity of cryftallized muriate of lead was thus obtained, until nearly the whole of the liquor was evaporated: to this laft portion a few drops of fulphuric acid were added, and the evaporation was carried on to drynefs; after which the refiduum, being diffolved in boiling diftilled water, left a fmall portion of fulphate of lead.

and by fubfequent evaporation muriate of *lead*.

The cryftallized muriate of lead was then diffolved in boiling water; and, being precipitated by fulphate of foda, was added to the former portion, was wafhed, dried on a fand-bath, and then weighed 120.20 grains.

D. The filtrated liquor was now of a pale bluifh-green, which changed to deep blue, upon the addition of ammonia; fome ochraceous flocculi were collected, and, when dry, were heated with wax in a porcelain crucible, by which they became completely attractable by the magnet, and weighed 2.40 grains.

Ammonia threw down a little *iron*.

E. The clear blue liquor was evaporated nearly to drynefs; and, being boiled with ftrong lixivium of pure potafh, until the whole was almoft reduced to a dry mafs, it was diffolved in boiling diftilled water; and the black oxide of copper, being collected and wafhed on a filter, was completely dried, and weighed 32 grains.

The blue liquor evapor. and boiled with potafh and then diffolved in water gave black *copper*.

200 grains of the ore, treated as here ftated, afforded,

Products.

	Grains.
A. Sulphur - - - -	34.
B. Oxide of antimony - -	63.
C. Sulphate of lead - -	120.20
D. Iron - - - -	2.40
E. Black oxide of copper -	32.

But the metals compofing this triple fulphuret are evidently in the metallic ftate; and white oxide of antimony precipitated

from muriatic acid by water, is to metallic antimony as 130 to 100; therefore, the 63 grains of the oxide muſt be eſtimated at 48.46, grains of the metal.

Again, ſulphate of lead is to metallic lead as 141 to 100; therefore, 120.20 grains of the former are = 85.24 grains of the latter. And, laſtly, black oxide of copper contains 20 *per cent.* of oxygen; conſequently, 32 grains of the black oxide are = 25.60 grains of metallic copper.

Component parts.

The proportions for 200 grains of the ore, will therefore be,

Sulphur - - - -	34.
Antimony - - - -	48.46
Lead - - - - -	85.24
Iron - - - - -	2.40
Copper - - - -	25.60
	195.70
Loſs -	4.30

Or, *per cent.*

Sulphur - - - -	17.
Antimony - - - -	24.23
Lead - - - - -	42.62
Iron - - - - -	1.20
Copper - - - -	12.80
	97.85
Loſs -	2.15

Theſe proportions, I have reaſon to believe, are tolerably exact; for I did not obſerve any eſſential variation in the reſults of two other analyſes, which I made of this ſubſtance, with every poſſible precaution.

The loſs may be principally aſcribed to the oxide of antimony and ſulphate of lead; but eſpecially to the former, which has a great tendency to adhere to filters and glaſs veſſels.

In ſome of the preliminary experiments, I obtained a ſmall portion of zinc; but, having received, through the kindneſs of Mr. R. Phillips, of Lombard-ſtreet, ſome pure cryſtals of the ore, I found that the zinc had proceeded from blende, which was imperceptibly mixed in the ſpecimens which I had firſt examined.

Deſcription

IV.

Defcription of a compound Electrical Inftrument for condenfing and doubling: with Experiments. By Mr. Wilson.

London, Auguft 10, 1804.

To Mr. NICHOLSON,

SIR,

I TAKE the liberty of troubling you with the following account and drawing of a compound condenfer of electricity, which I think is an improvement on Cavallo's multiplier of electricity, defcribed in his Treatife on Electricity in the third volume, of the fourth edition, and if you think it worth a place in your Philofophical Journal, you will very much oblige me by inferting it therein.

Compound electrical condenfer.

I am your obedient
Humble fervant,
W. WILSON.

The drawing is an exact reprefentation of the inftrument, about half its real fize, *(Plate* 3.*)* The plates A, B, C, D, and E are fupported by glafs fticks, covered with fealing wax at the upper part; but the plate F is fupported by a wire, which has a joint at the bottom, by which it may be brought near the plate E, or thrown back away from it. 3 is a fcrew, which ftops againft the glafs fupport of E, and regulates the diftance of the two plates when one is brought near the other. The plates A, C and E are fixed to the bottom board, and B and D to the levers L l, which move round the pins p p, and and are connected by the rod R. It will be feen by the drawing, that when the lever L is moved, the plates B and D are moved in contrary directions, that is, they both approach towards or recede from the plate C at the fame time, but on oppofite fides. The faces of the plates are ground flat, and they are fo adjufted that if the fcrews 2 2 did not ftop the levers when they are moved, the face of B would ftop flat againft the face of A, and fo would the face of D againft C. The fcrews 2 2 ftop them when at a very fmall diftance, which may be made more or lefs by turning the fcrews.

Defcription and drawing of the inftrument.

 This

Its ufes as a condenfer, a fingle and double multiplier, and a doubler of electricity.

This inftrument ferves the purpofe of a condenfer, a fingle and double multiplier, and a doubler of electricity. When it is ufed as a fingle condenfer, the plate B is brought as near A as the fcrew 2 will permit, and then the wires *b* and *f* touch one another and uninfulate B, which increafes the capacity of A. As the capacity of A is increafed it will receive a greater charge of electricity from any electrified body brought into contact with it. Confequently, when B is removed, the charge on A will be much more intenfe than it would have been if the plate B had not been oppofed to it; but this intenfity will not be greater than that of the electrified body, if its furface is not greater than the furface of the receiving plate A; for a condenfer does not increafe the quantity of electricity, it only collects it into a fmaller fpace than it was before. So that if the electricity of a very fmall body is required to be afcertained, the fimple condenfer will not anfwer the purpofe. Some other means muft be ufed when this is the cafe, and Cavallo's multiplier is the leaft exceptionable of the inftruments ufed for this purpofe. The inftrument reprefented in the drawing forms a multiplier, either fingle or double, for when B is near A and uninfulated, A will receive a much greater charge that it would if B had not been near. And becaufe B is uninfulated, A will induce a contrary ftate on B, and of nearly equal intenfity; which ftate is preferved: For the inftant the lever is moved to carry B farther from A; the contact between the wires *b* and *f* is difcontinued, and B infulated, and as it removes farther away from A, the intenfity of its charge will increafe the fame as that of A will, and be ready to part with nearly the whole to another condenfer.

Now as the plate B removes away from A, the plate D approaches C, and when at a fmall diftance from it, the wires *d* and *g* come into contact, which operation uninfulates D, and therefore caufes the two plates C and D to form a condenfer; and at the fame inftant that *d* touches *g*, the wires *b* and *c* touch one another, fo that nearly the whole of the charge of B is communicated to C, which induces a contrary ftate on D, as the charge of A did on B. D will therefore be in the fame ftate as A is, which ftate is preferved; for the inftant the plate B removes away from near C, the plate D alfo removes away from it, and the contact between the wires *d* and *g* is difcontinued, and D infulated; and by continuing the motion the plate

plate B will come near A, and D will remove away from C till the wires *b* and *f* touch one another, at which inſtant the wire *d* touches the wire *e*. Now if the plate E and F are near one another, they will form a third condenſer, and nearly the whole of the charge of D is communicated to E in the ſame manner the charge of B was communicated to C; at the ſame time B recruits its charge by coming near A, which will be communicated to C, which induces a contrary ſtate on D, and which D communicates to E, and this operation is repeated every time the lever is moved backwards and forwards; ſo that the charge on C is increaſing at every motion of the lever, while the charge on A remains the ſame; and when C becomes ſo much charged as not to be capable of receiving any more from B (which will be the caſe in a certain number of motions) the operation will ſtill go on between D and E, the charge on E increaſing while the charge on C remains the ſame, ſo that E will acquire a charge as much greater than C as the charge on C is greater than the charge on A, which will be manifeſted by removing the plate F away from E. When the wire *a* is ſcrewed into the plate A, and connected with E by means of the wire 4 5; the inſtrument then poſſeſſes all the properties of the double of electricity, for then all the charge communicated to E (which is of the ſame nature as that of A) will be communicated to A, which will continually increaſe the intenſity of its charge, and that will have an increaſed effect on B, &c. at every motion of the lever. There is no limits to this accumulation, but that where the charge is ſo intenſe as to paſs from one plate to the other in the form of a ſpark.

Its uſes as a condenſer, a ſingle and double multiplier, and a doubler of electricity.

I have made many experiments with this inſtrument relative to its ſpontaneous electricity, and I find, as a ſingle multiplier (that is when an electrometer is connected with C) it has no effect on the moſt delicate gold leaf electrometer I could make; but as a double multiplier (that is when an electrometer is connected with E) there is ſome ſmall effect if ſome electricity had been communicated to it within an hour or two, although it may have been diſcharged by touching each of the plates with a metal point (which I find is the moſt effectual way of diſcharging ſmall portions of electricity,) but if it has ſtood three or four hours after being diſcharged, it will not give any ſigns of electricity. When uſed as a doubler it always becomes electrified with between eight and ſixteen motions of the lever,

On the ſpontaneous electricity of this inſtrument.

On the ſpontaneous electricity of this inſtrument.

even though it has not been uſed for two or three months.— But if it has been uſed within two or three hours, the effect will take place with fewer motions of the lever, and if it has been uſed within a few minutes, two or three motions will be more than ſufficient to aſcertain the quality of the electricity. It is to be obſerved that the inſtrument was always diſcharged (by the metal point as before obſerved) between each trial.

In the courſe of making the experiments on the ſpontaneous electricity of this inſtrument I found that it was always poſitive if the inſtrument had not been uſed for two or three days, whatever electricity it was laſt charged with. But the time it muſt ſtand unuſed after it has been diſcharged, to produce this effect, depends a great deal on the weather; if the air is very humid, twenty-four hours is quite ſufficient, but if it is very dry, it will require four or five days.

After I had obſerved that after the inſtrument was charged with poſitive electricity its ſpontaneous electricity was always poſitive; and that after it was charged with negative electricity, its ſpontaneous electricity was negative only within a certain time after it had been diſcharged, and then became poſitive; and alſo that it required a greater number of motions of the lever to produce a certain effect on the electrometer with negative electricity, the longer it ſtood after it had been charged with negative electricity, and that when it became poſitive, the longer it ſtood the leſs number of motions of the lever it required to effect the electrometer with poſitive electricity to a certain degree, and this within ſome certain limits. I was at a loſs how to account for this change. However, after ſome conſideration, I began to ſuſpect that the plates (although all of the ſame metal, copper) had each a property peculiar to itſelf, of acquiring a certain ſmall charge of one kind of electricity in preference to the other; and that if they were left to themſelves they would naturally do ſo. I therefore began a ſet of experiments to aſcertain the probability of this ſuppoſition. Firſt, directly after the inſtrument had been uſed for negative electricity, I diſcharged it by touching each of the plates with a metal point, which I held in contact with each plate for two or three ſeconds. An electrometer was then connected with the plate A, and while it was in this ſituation, it was made to diverge with poſitive electricity;* it was then

* The electrometer was made to diverge by bringing an excited glaſs or ſealing wax near it.

dif-

difcharged by a flight touch with the finger, the lever was then moved backwards and forwards about fourteen times, and the electrometer diverged with negative electricity. This was repeated feveral times with the fame effect.

On the fpontaneous electricity of this inftrument.

The lever was then put in fuch a pofition that the plates could not be fuppofed to act as condenfers, one pair more than another, and the whole was left untouched for twenty-four hours. It was then tried, and with twenty motions of the lever the electrometer diverged with pofitive electricity.

It was then left untouched for eighteen hours more, and on trial the electrometer diverged with pofitive electricity. The inftrument was next difcharged with the metal point as before, and the electrometer made to diverge with negative electricity, which was difcharged by a flight touch of the finger. The lever was now worked, and with nine motions of the electrometer diverged with pofitive electricity. It was then difcharged and left untouched for fixteen hours, after which it diverged with pofitive electricity after a few motions of the lever. It was then left untouched for five days, after that time it gave pofitive electricity when the lever was worked. Thefe experiments I have repeated a great many times with the fame effect.

As it appeared from the foregoing experiments that the refiduum on the electrometer was infufficient to overcome the effect of the refiduum on the plates, I was induced to charge the whole inftrument with the effect of the electricity, I would wifh to communicate to it, and this I did by making the electrometer diverge with the defired electricity, and while it was divergent I began working the lever, to communicate the effect to all the plates. I charged the inftrument with negative electricity firft, and difcharged it with the metal point. The lever was then worked, and in eight motions the electrometer diverged with negative electricity. It was then left untouched for five days, when eighteen motions of the lever made the electrometer diverge with pofitive electricity. This has been often repeated with the fame effect.

The inftrument was next charged with pofitive electricity, and difcharged as before, after which, fix motions of the lever made the electrometer diverge with pofitive electricity. It was left untouched for twenty-four hours, when the elec-

trometer

On the fpontaneous electricity of this inftrument.

trometer diverged with pofitive electricity with twelve motions of the lever, and fo it did after it had ftood untouched for fix days.

The probability of the foregoing fuppofition concerning the property of the plates acquiring a charge of one electricity in preference to the other, was more ftrengthened by the following experiment, which has been repeated feveral times. The inftrument was charged with negative electricity, and then left without difcharging for twenty-four hours, when fixteen motions of the lever made the electrometer diverge with pofitive electricity.

I make no doubt if the plates were of different metals this effect would be more ftriking, and that in fome cafes we fhould have the contrary electricity. I intend to conftruct one of thefe inftruments in fuch a way that plates of any metal may be put on, and their effect tried. I think this a fubject well worth purfuing as it may throw fome light on fome of the phenomena of electricity, which have their caufes at prefent buried in obfcurity.

V.

Note refpecting the Sufpenfion of Zinc in Hidrogen, and the confequent Ignition and Fufion of Platina Wire. By AMICUS.

To Mr. NICHOLSON.

DEAR SIR,

Ignition and fufion of platina in Cuthbertfon's gazometer,

I EMPLOY a *gafometer* of a much more fimple conftruction and much lefs expence than Lavoifier's, which that fkilful and intelligent artift, *Cuthbertfon,* invented at my entreaty, about ten years ago. It is defcribed in your valuable Journal (quarto feries, vol. II. p. 235.) The brafs rod or the thick wire, which conveys the electric fire through the upper receiver, has about half an inch of platina wire appended, to hang juft over the aperture through which the hydrogen gas rifes at the bottom in the brafs work. Zinc of any other fort of metal would either be melted or readily oxided. But the other day, I was much furprized to fee the platina wire, on becoming as ufual *red hot* from the flame of the hydrogen gas, melt into globules as

as readily as copper or brafs wire would have done. I foon perceived from the *colour* of the flame of the hydrogen and the depofit of *white clouds* on the infide of the glafs, that the rapid folution of the finely powdered zinc ufed to afford the gas, that this metal was actually fufpended or diffolved in the gas, and hence the zinc acting to the platina fufed the latter. It may be perhaps ufeful to know this fact, which occurred during the public lecture. —caufed by the volatilization of zinc, &c.

Your's, as ufual,

AMICUS.

July 2, 1804.

VI.

Defcription of a Blow-Pipe acting by the Preffure of Water, by the Abbé Melograni. In a Letter from Mr. G. B. GREENOUGH.

To Mr. NICHOLSON.

SIR,

No. 15, *Parliament-Street,*
Auguft 13, 1804.

PERMIT me to make you acquainted with the principle of an inftrument invented by the Abbé Melograni, and ufed by him at the Royal Mineralogical Collection at Naples, as a fubftitute for the blow-pipe. I regret that I cannot fend you an accurate account of its conftruction, as I made no fketch at the time, and fpeak only from recollection. Blow-pipe fupplied by two veffels alternately changing their pofitions.

Two hollow glafs globes of convenient fize were connected together by two brafs tubes laid along-fide each other, one of them having a valve or ftop cock, I am not fure which, at each end, and a fide tube of the fame metal going off from the middle at right angles. The frame was attached to the vertical tubes, fo as to allow the globes to circumvolve each other. Let the lower globe be half filled with water and inverted; Then the water in A *(Fig.* 2. *Plate* IV.) running into B through the tube C, will force a conftant ftream of air into the tube D, and thence, the upper orifice being clofed, into the fide-tube E, at the mouth of which the candle is placed. When the water is nearly run out invert the globes.

If

If the principle of this inſtrument ſhall appear to you deſerving of the public attention, it would give me pleaſure to ſee it ſubmitted to the readers of your excellent Journal. I have the honour to ſubſcribe myſelf,

Sir,

Your obedient humble ſervant,

G. B. Greenough.

VII.

Outlines of the Mineralogical Syſtems of Romé de Liſle and the Abbé Hauy; with Obſervations. By the Abbé Buée. Communicated by the Hon. Robert Clifford.

To Mr. NICHOLSON.

SIR,

Outlines of the mineralogical ſyſtems of Romé de Liſle, and the Abbé Hauy.

WHEN the public mind appears at any time to direct itſelf towards a particular ſcience (as it does at preſent towards mineralogy,) no perſon can feel the neceſſity of removing any erroneous opinions relating to that ſcience, more forcibly than a gentleman who dedicates his time, as you do, Sir, to propagate correct ſcience by means of your Journal. Actuated by the ſame ſentiments, may I requeſt the inſertion of the encloſed paper, whoſe object is to remove ſeveral erroneous opinions relating to the mineralogical ſyſtems of Romé de L'Iſle, and of the Abbé Hauy. It was written by the Abbé Buée in French for a friend of his, but on its being communicated to me by the author, I requeſted leave to ſend an Engliſh tranſlation of it to you, thinking it might be acceptable to thoſe who are fond of philoſophical purſuits. Should your opinion coincide with mine, you will certainly afford it a place in one of your enſuing numbers.

And I remain, Sir,

Your very humble ſervant,

Robert Clifford.

Welbeck-Street, July 13, 1804.

A Letter

A Letter from the Abbé Buée to Mr. —, on Mr. Romé de L'Isle's and the Abbé Hauy's Theories of Crystallography.

SIR,

IN consequence of your request, I send you the parallel of the two Theories of Crystallography, which seem to divide mineralogists in this country, those of Mr. Romé de L'Isle and of the Abbé Hauy. You are perfectly acquainted with the former theory, but nearly a stranger to the latter. Having lived for six and thirty years in habits of intimacy with the Abbé, I dwell with pleasure on his works, and will do my utmost to satisfy your curiosity.

Parallel between the theories of Romé de l'Isle and Hauy.

To Mr. de L'Isle is due the merit of having called the attention of naturalists to that neglected branch of mineralogy, crystallography; of having discovered that that branch, though neglected, was perhaps the most interesting part of mineralogy, and the only part which could raise it to the dignity of a correct science; in short, of having discovered order, by numerous observations as ingenious as new, there, where a Cronstet, a Bergman, a Buffon, or a Kirwan could perceive nothing but confusion; and thus seemed to rescue nature from the charge of caprice, almost imputed to it because great mineralogists had neglected to study its unerring laws.

De l'Isle first exhibited order in crystallography.

It was exclusively reserved to the Abbé Hauy to point out, to explain, and apply those laws. He demonstrated where De L'Isle affirmed. He discovered those *hidden facts*, which he has since shewn to be the mathematical consequences of facts observed by De L'Isle. If the latter furnished a part of the materials, the Abbé has augmented and employed them. The discoveries of these two writers force me to subdivide crystallography into two distinct parts; descriptive and philosophical; and under these two heads I will rapidly describe the labours of each author.

Hauy first explained the laws of crystallization.

Descriptive.—The most important part of Mr. de L'Isle's work consists in his crystallographical tables. In each of these tables (seven in number) he describes one of the principal forms assumed by crystals, and then delineates the different modifications of which that form is susceptible, by means of different troncations *(troncature)* as he calls them.

Descriptive crystallography. De l'Isle's tables.

For elucidation, let us take a cube, the primitive form of the second table. A cube, it is known, has six faces, eight solid

Modifications of crystals described by troncations. Instance the cube.

ſolid angles, and twelve edges. If the cube be truncated in a parallel to one of its faces, a rectangled parallelipipedon will be produced, and the equality of the faces will be deſtroyed.

If the eight ſolid angles of the cube be ſtruck off, eight new faces will replace the eight ſolid angles, and in place of ſix ſides we ſhall have fourteen. If the twelve edges be taken off, twelve new faces will ſucceed the ſtrait lines, and the ſolid will have eighteen ſides. Such are De L'Iſle's *ſimple troncations.* They may be then combined with each other, and made more or leſs deep; hence an immenſe variety of new figures. But theſe new forms again may be truncated in the directions either of their faces, ſolid angles, or edges, and theſe new troncations more or leſs deep, called by De L'Iſle *ſur-troncatures,* may alſo be combined with each other. Here the forms muſt multiply to infinity, and their boundleſs numbers will ſoon bury the primitive cube in oblivion.

It muſt not be ſuppoſed that nature has furniſhed us with this infinite ſeries of forms; indeed Mr. De L'Iſle in his tables has only mentioned thoſe he had obſerved, with ſome few additional ſuppoſitious figures, of which ſeveral have been ſince diſcovered to exiſt.

Account of De l'Iſle's ſeven tables of cryſtals.

This ingenious naturaliſt has given us, as I have already ſaid, ſeven cryſtallographical tables. In the 1ſt he deſcribes the tetrahedron and its modifications; in the 2d the cube; in the 3d the rectangular octahedron; in the 4th the rhomboidal parallelipipedon; in the 5th the rhomboidal octahedron; in the 6th the dodecahedron, with triangular faces, and to each are ſubjoined their reſpective modifications. The object of the 7th table is to point out certain modifications of the octahedron and parallelipipedon, whether rectangular or rhomboidal. Plates accompany each table, where the figures are drawn, and in the obſervations and notes on them are to be found the meaſures of the principal angles.

They contain general ſolids; which are ſubſequently applied to the cryſtals of nature, viz. *ſalts, ſtones, and metals.*

Theſe cryſtallographical tables exhibit only general repreſentations of ſolids, which Mr. de L'Iſle in the courſe of his work applies to the different cryſtals which had already been diſcovered, and fallen within his obſervation. His work conſiſts of three parts. In the firſt he treats of ſaline cryſtals; in the ſecond of ſtoney *(pierreux)* cryſtals, and in the third of metallic cryſtals. Thoſe of the firſt claſs are artificial, thoſe of the two latter claſſes are natural cryſtals, and are ſubdivided into genera, ſpecies, and varieties.

When

When treating of a ſpecies or of a variety, he refers his reader to the table where the figure of that ſpecies or variety is to be found, and he then enumerates every thing relating to minerals aſſuming that cryſtalline form: But I cannot terminate this ſketch better than by the following extract from the Abbé Hauy's treatiſe on mineralogy.

His method of deſcription.

" In ſhort Romé de L'Iſle reduced the ſtudy of cryſtallography to principles more exact and more conſiſtent with obſervation. He claſſed together, as much as he was able, cryſtals of the ſame nature. From among the different forms belonging to each ſpecies, he ſelected one which appeared to him to be the moſt proper, on account of its ſimplicity, for the primitive form, and then ſuppoſing it to be truncated in different manners, he deduced the other forms, and eſtabliſhed a certain gradation or ſeries of paſſages from the primitive form to that of polyëdrons which would ſcarcely appear to have any connection with it. To the deſcriptions and figures which he gave of the cryſtalline forms, he added the mechanical meaſurement of the principal angles, and he ſhewed (a moſt eſſential point) that theſe angles were conſtantly the ſame in each variety. In a word, his cryſtallography is the fruit of immenſe labour by its extent; almoſt entirely new in its object, and of great value for its utility." (Vol I. page 17.)

Quotation exhibiting a ſummary of De l'Iſle's labours.

The Abbé Hauy in his treatiſe on mineralogy embraces a far greater extent than Mr. de L'Iſle. His mineralogy is not only deſcriptive, but it is phyſical, chymical and geometrical. In the perſuaſion that a mineral cannot be well deſcribed, nor even in many caſes recognized, unleſs its phyſical, chemical, and geometrical characters are clearly laid down, the Abbé never omits any one of thoſe characters when aſcertained, and expoſes with the moſt ſcrupulous exactneſs every thing relating to them that obſervation has authenticated. He has beſtowed particular attention to the electrical and magnetic phenomena, and has enriched the ſcience with a multitude of new and curious obſervations. He attentively examined the property of double refraction, which ſeveral tranſparent minerals enjoy; and here again he has extended the boundaries of ſcience. A few minerals were known to poſſeſs this property, and the Abbé has diſcovered it in ſeveral where it had never been ſurmiſed.

The mineralogy of Hauy is not merely deſcriptive, but phyſical, chemical and geometrical.

Electricity.

Magnetiſm.

Double refraction.

When we conſider that writers on mineralogy have hitherto grounded their ſyſtems *excluſively*, ſome on the exterior characters,

Hauy has excluded none of the habitudes,

or properties of minerals.

racters, others on the chemical properties of minerals, and that the Abbé really has, purfuant to his plan, (fee in the beginning of the volume of plates, *La diftribution methodique des mineraux, par claffes, ordres, Genres et efpeces,* the methodical diftribution of minerals into claffes, orders, genera, and fpecies) united all that has hitherto been difcovered on mineralogy, without falling into that confufion which has ever been imputed to other mineralogical writers, we are almoft aftonifhed at his fuccefs. "To clafs minerals, to furnifh the means of difcovering to which clafs, genus, and fpecies a mineral under examination belongs," are the two great problems which the Abbé Hauy propofes for folution.

Claffification, chemical: inveftigation by external characters.

He folves the firft in following Bergman's method (founded on chemical properties) much improved by the immenfe progrefs which chemical analyfis has made fince the days of that great chemift. In the folution of the fecond he follows Werner's method (grounded on exterior characters) but corroborated by a multitude of new experiments, eafily made and brought to a furprizing degree of correctnefs by the Abbé's own labours on the forms of cryftals. But I perceive that the immenfity of matter contained in this treatife is leading me from that point which I had particularly in view, I mean cryftallography.

Hauy's defcription of cryftals, 1. by delineation.

In the defcription of cryftals the Abbé employs three different means. 1ft He draws their figure; he does not give cryftallographical tables as Romé de L'Ifle, which are only general properties, but draws feparately each fpecies and variety. Every form given in the plates has been examined by himfelf; he has calculated every angle, and neverthelefs his plates contain one third more figures than De L'Ifles tables.

2. By fymbolic figns defignating the laws of their production.

2d He makes ufe of fymbolic figns, than which nothing can be more fimple, and were invented not to recall the form of the cryftal to the mind, but the laws by which it has been produced. Yet I have met with perfons, who were fo accuftomed to thefe figns, that at firft fight of them they could immediately figure to themfelves the form of the correfponding cryftals. Thefe figns can alfo be fpoken, and much circumlocution in confequence avoided.

3. By nomenclature.

3d A fignificant nomenclature fubdivided into general and particular. The general is for the *minerals*, and comprifes only fubftantives; the particular for the *cryftals*, and is entirely compofed of adjectives. He ftudioufly avoided

introducing new names, and neverthelefs has been obliged to introduce many, where new fubftances, names capable of giving falfe impreffions, or others void of fignification and unfupported by long ufage, required it. He then fubftituted names taken from the Greek; a language, he fays, that eminently enjoys the faculty of combining feveral words together, fo as to form one reprefenting concifely the object to be named.—The adjectives ufed in the nomenclature of the cryftals alfo allude to fome remarkable circumftance of the cryftalline form.

Philofophical cryftallography.

I fhall now proceed to philofophical cryftallography, which might be called the philofophy of mineralogy. It does not confift in fearching for the primary caufes of phenomena, nothing can be lefs philofophical than fuch a refearch; primary caufes will ever be beyond the reach of the human mind! The immortal Newton was the firft to point out to us by the method followed in his admirable book of the Principia, that the only true philofophical way of treating a phyfical fcience, or of explaining a natural fact, was to demonftrate that it was the mathematical confequence of a general law, grounded on an aggregate of facts already obferved and capable of correct calculation. If any one of thefe conditions are wanting, we immediately launch out into hypothefis, explanations become vague, and however much we may be perfuaded of the truth of our affertions, we can acquire no certainty.

The true method of explaining phyfics is to fhew that the facts are mathematically deducible from a general law.

Hypothefis.

The method of Rome de l'Ifle fhewn to be deficient in correctnefs.

Let us apply thefe principles to our two writers. De L'Ifle, in declaring that the various forms obferved in cryftals of the fame fubftance are only modifications of one conftant primitive form, certainly announced a moft important truth. It was a flafh of genius; but in a philofophical enquiry, to prove it and not fimply to fay it, was the neceffary ftep. On the firft infpection of his cryftallographical tables, a ftudent is tempted to think that important truth demonftrated; but on a clofer examination, the impreffion is done away. The fame order pervades every table. By flight paffages the ftudent is led from the fimpler to the more compound forms, and after every paffage, is tempted to fay; this can only be a modification of the primitive; then when the real cryftals, and the figures of the tables are compared together, and all thofe of the fame fpecies (with a very few exceptions) are found in the fame table, how eafy it is to perfuade ourfelves that nature *muft* operate by

fimilar

ſimilar paſſages when producing the various forms of cryſtals; and the primitive of the table before us *muſt* be the primitive of the cryſtal under examination. In a word it is the moſt ſimple form; and firſt impreſſions greatly ſtrengthen the illuſion. If perſuaſion was the ſole object of philoſophy, De L'Iſle would have been a powerful philoſopher; but philoſophy muſt convince, demonſtrate, and wreſt conſent, however violently oppoſed. An enemy muſt not therefore be able to make uſe of the ſame arms, or adduce the ſame proofs to eſtabliſh a contrary opinion. Nevertheleſs ſuch would be caſe with Mr. De L'Iſle's tables and the application of them: For it is an inconteſtable fact, that by a ſeries of arbitrary troncations we may paſs inſenſibly from any given form to any other. Grounded on this principle, and ſeconded by Mr. de L'Iſle's ingenuity, any form may become primitive, and any other deduced from it. Now as the combinations are infinite, a multitude of tables may be conſtructed, forms of the ſame ſpecies may be diſperſed in different tables: the moſt ſimple of each table, will be the primitive, therefore forms of the ſame ſpecies will have different primitives. But when by the ſame principle both ſides of the queſtion can be proved, nothing is proved.

and may be employed to eſtabliſh falſhood.

On the moſt ſimple form.

To ſay the moſt ſimple form muſt be the primitive is an illuſion, for we know not what is the moſt ſimple for nature. With our feeble organs and confined ſenſes we can form no judgment of *ſimple*, when the operations of nature are in queſtion. Nature embraces the entire univerſe, her laws are ſimple; but the combinations made according to thoſe laws are unbounded, therefore complicated.

The ſyſtem of troncations is the moſt obvious.

Let us not forget, however, that the idea of troncations, and the idea of taking the moſt ſimple form for the primitive, are ſo natural that they muſt have been the firſt to preſent themſelves to the man who was opening the carreer: "Often, ſays the Abbé Hauy, (vol. I. page 14) a more compound form only differs from a more ſimple one by certain little faces which may be produced by ſections, either at the ſolid angles, or on the edges of the ſimpler form," and in a note he ſays, "this was the obſervation which gave the celebrated Romé de L'Iſle the idea of his ſyſtem of troncations, that he might ſucceſſively deduce from each other the different varieties of cryſtalline forms aſſumed by the ſame ſubſtance."

Mr.

Mr. De L'Isle terminates the introduction to his work by certain axioms, as he styles them, the 2d and 16th are as follow: De l'Isle's proposed axiom.

"II. Every angular polyhedron, or every crystallized substance is a SALT in the most extended acceptation of that term."

"XVI. Every saline substance whose constituent parts are perfectly saturated and combined affects the *cubic* form, or its inverse the *octahedron;* whereas the salts which are not neuter, or whose constituent parts are not exactly combined, affect either the *prismatic* or the *rhomboidal* forms."

I need scarcely observe that, to treat these axioms only as doubtful, would be treating them kindly. The other axioms are matters of fact, from which he draws no consequence, and indeed it would have been difficult for him to have drawn any.

The Abbé Hauy does not undertake to prove generally, that among the different crystalline forms of the same substance, one of them is the primitive; but he produces that primitive form from each crystal, which is always similar in similar substances. He demonstrates it analytically and synthetically; by an analysis which might be called *mineralogical analysis,* and pointed out by nature herself: By a synthesis hitherto the property of mathematicians, but here supported by the general laws which his analysis has revealed to him. The constant accord found between this synthesis, and daily observation is a proof of the exactness of his method. Hauy's exhibition of the primitive form in crystals, by analysis and synthesis.

Two facts were the foundation of his theory: 1. In all times jewellers and lapidaries have remarked that stones are more easily cut in some certain directions than in others. 2. Whoever has been in the habit of seeing natural crystals must have observed, that when their forms are well determined, they are always terminated by plane surfaces. Thus, says the Abbé, "those soft outlines, and that roundness so frequent in the animal and the vegetable kingdom, where they are inherent to the organization, and contribute even to the elegance of the forms, indicate on the contrary in minerals a want of perfection. The characteristic of true beauty in minerals is the strait line, and it was with truth that Romé de L'Isle declared *that line* to be the peculiar property of the mineral kingdom." The first fact suggested the mineralogical analysis, and the second furnished him with the laws on which he grounded his synthesis. Fundamental facts. 1. Crystals may be cleft in certain directions only; 2. their boundaries are right-lined or plane.

Enquiries on the firſt Fact.

On the analyſis or ſubdiviſion of cryſtals into regularly formed portions by fiſſure.

1. All cryſtals that can be ſplit by means of inſtruments, offer to the view, if ſplit in certain directions, plane and ſmooth ſurfaces. If divided in other directions, the fracture is rugged. I uſe the word *ſplit*, and not *ſawed* or *cut*, as the ſections of the cryſtal are not to be obtained by ſlow and continued efforts, but by ſudden ſhocks. Patience, dexterity, and habit enabled the Abbé to ſplit a great number of cryſtals; in all he diſcovered plane ſmooth ſurfaces when ſplit in certain directions, but when in other directions, the fracture was always rugged and irregular. I requeſt, Sir, your attention to this important fact, it is fundamental, and the more important, as ſeveral perſons of much general information have neglected to attend to it, and, in conſequence, have ſuppoſed the whole of this theory to be grounded on hypotheſis. It would be equally erroneous to confound theſe ſections of cryſtals with De L'Iſle's troncations. The latter indeed warns his readers, that by the word *troncations* he wiſhes only to figure the appearance of the cryſtal examined. They are not therefore real, but only a means of warping the imagination to the exterior form of the cryſtal, and are by their nature only deſcriptive. The Abbé Hauy's ſections are real, and are pointed out to the obſerver by the interior ſtructure of the cryſtals; they are experimental.

It is *experimental*, whereas the troncations of De l'Iſle were *hypothetical*.

The inclinations of the faces are conſtant.

2. The plane ſmooth ſurfaces obtained by the above method are reſpectively parallel to 3, 4 or 6 planes. The mutual inclination of theſe planes to each other are conſtant in cryſtals of the ſame ſubſtance, whatever may be the exterior form of the cryſtal. Native antimony, phoſphate of lead, and quartz ſeem to ſhew an appearance of more than ſix planes, and the Abbé Hauy leans to the opinion of only five planes in ſome caſes; but as theſe are exceptions to the general rule, and would only tend to complicate this ſtatement, I ſhall take no further notice of them.

Explanation of the figures and formation of cryſtals by the method of Hauy.

Let us ſuppoſe the ſmooth ſurfaces to be only parallel to three planes, or in other words, that the ſubſtance will only ſplit in three directions, in that caſe the ſections can only produce a parallelipipedon, whoſe nature is determined by the mutual inclination of the planes to each other. If the planes are perpendicular, it will be rectangular, &c.

We next ſuppoſe the ſmooth ſurfaces to be parallel to four planes. Here a diſtinction ariſes, whether three of theſe planes

Explanation of the figures and formation of cryſtals by the method of Hauy.

have a common interſection or not; and it muſt be remembered that if the four planes have a common interſection, no ſolid can be produced, as they can neither bound nor include a ſpace. If therefore three of the four planes have a common interſection, the ſplittings will produce either one hexahedral priſm, or three parallelipipedons, which will be ſimilar or diſſimilar, according to the ſimilarity or diſſimilarity of inclination of the planes, or one triangular priſm. On the contrary, if the four planes only interſect each other two and two, there will be produced either one octahedron, or four parallelipipedons, or one tetrahedron.

Laſtly, let us ſuppoſe the ſmooth ſurfaces to be parallel to ſix planes; then there ariſe an immenſe number of caſes.—But we will for the preſent confine ourſelves to the only caſe that has hitherto been obſerved in nature: Where the interſection of the planes is two and two, then we obtain either, 1. dodecahedron with pentagonal, quadrilateral or triangular ſides, according to the ſections made, or fifteen octahedrons, or twenty parallelipipedons, or fifteen tetrahedrons. It may be proper to obſerve here that though the ſections parallel to the ſix planes may be clearly indicated, nevertheleſs it rarely happens they can all be executed, but it will ſuffice for the purpoſes of geometry that they be clearly indicated to render the conſequences drawn from them mathematically correct.

Having laid down theſe premiſes, let us proceed to the diſſection of a cryſtal of carbonate of lime (the ſpath calcaire of De L'Iſle) whoſe primitive form is a rhomboid or a parallelipipedon bounded by rhombs. Hitherto ſections have only been obtained in the three directions parallel to its ſides. If theſe ſections be directed ſo as to always paſs through the center of two oppoſite ſides, they will produce eight rhomboids equal to each other, and ſimilar to the original one. The ſame operation may be repeated on each of theſe eight rhomboids, and continued ſo long as the ſubſtance remains carbonate of lime, that is to ſay, to be a combination of 55 parts of lime, 34 of carbonic acid, and 11 parts of water of cryſtallization:—(ſee Bergman.) But this diviſion of the cryſtals into ſimilar ſolids has a term, beyond which we ſhould come to the ſmalleſt particles of the body, which could not be divided without chemical decompoſition; that is to ſay, without an alteration

 in

Explanation of the figures and formation of cryſtals by the method of Hauy.

in the proportions of lime, carbonic acid, and water. Theſe laſt particles which are ſtill rhomboids, are what the Abbé Hauy calls the *integrant particles* of the carbonate of lime. In the ſuppoſition therefore that a rhomboid of this ſubſtance can only be divided in three directions, by ſections parallel to the ſides, it is evident that the integrant particles muſt be ſimilar rhomboids.

If a cryſtal can be divided by ſections in more directions than three, what will be the form of the integrant particles? For example, in the phoſphate of lime (the chryſolite of De L'Iſle) where the ſections are parallel to four planes, three of which have a common interſection. According to what has been ſaid above, theſe ſections can produce either one hexahedral priſm, or three parallelipipedons, or one triangular priſm. It is evident that by carrying the diviſion, according to thoſe ſections, to its greateſt length, either the laſt hexahedral priſm, or the laſt three parallelipipedons, or the laſt triangular priſm, will be produced. Are theſe laſt ſolids the integrant particles; are each of them ſo; or is there only one of them entitled to that denomination; and if only one, which of them? My anſwer is, only one of them; and that one, the triangular priſm, which may be proved thus.

It cannot be denied that the integrant particle is that little ſolid which contains the leaſt poſſible quantity of the body, without affecting the chemical compoſition of the ſubſtance. This granted, let us ſuppoſe the hexahedral priſm to be the integrant particle. In that ſuppoſition the laſt triangular priſm muſt contain the laſt hexahedral priſm, and is equal to the latter more three little triangular priſms, or in other words to nine ſimilar triangular priſms, while the hexahedral priſm only contains ſix. But the laſt triangular priſm and the laſt hexahedral priſm each contain an exact proportion, and therefore a ſimilar proportion of chemical component parts; therefore their difference alſo contain an exact proportion; but it is impoſſible to conceive how their differences can contain the exact proportion, unleſs each of the three little triangular priſms alſo contain it, they muſt therefore contain it, and each of them muſt be an integrant particle; therefore the hexahedral priſm cannot be one; neither can the parallelipipedons be integrant particles, as the ſame arguments will ſtand good againſt them which have been applied to the hexahedral priſm; there-

fore

fore the triangular prifm muft be the integrant particle:* "The forms of the integrant particles, fays the Abbé (vol. I. page 30) may be reduced to three, the tetrahedron or the moft fimple of pyramids, the triangular prifm or the moft fimple of prifms, and the parallelipipedon or the moft fimple of folids, having parallel fides two and two, and as four fides are neceffary to circumfcribe a fpace, it is evident that the above three forms in which the number of fides are fucceffively four, five and fix, are again in this point of view the moft fimple poffible."

Explanation of the figures and formation of cryftals by the method of Hauy.

The phofphate of lime or chryfolite is a fubftance that has given rife to much curious anecdote. It fhews in what a ftate Abbé Hauy found the mineralogical nomenclature, and points out the accuracy of his analytical method. Achard, a chemift at Berlin had analyfed the chryfolite, and publifhed that it contained, of filex 15 parts, alumina 64, lime 17, and of iron one. This ftartled the celebrated Vauquelin, who had feen Klaproth's analyfis of the chryfolite (the apatite of Werner) containing of lime 55 parts, and of phofphoric acid 45, (probably the water of cryftallization is added to the acid.) A Frenchman of the name of Launoy fent a quantity of this fubftance to Paris, fome of it was purchafed by the *Ecole des Mines,* and Vauquelin was defired to analyfe it. The latter foon fufpected Achard had been mifled by the name, and had not obtained the proper fubftance, a miftake the more eafily made as, fays Vauquelin, "the name of chryfolite was given to a great variety of ftones, fuch as the *peridot,* the *chryfoberil,* the *olivine,* (fince found to be the fame as the *peridot),* and in general to *ftones* having a *yellow colour.*"

He foon difcovered the chryfolite fent from Spain contained lime and phofphoric acid. "I had no fooner made this difcovery, fays he, than I enquired of Abbé Hauy whether he had compared the integrant particles of the chryfolite with thofe of the apatite or cryftallized phofphate of lime. He anfwered me that he had not made the comparifon, but that he would get his papers on primitive forms, (this was four years before the publication of his work) fee what notes he had

* Therefore the fections producing the hexahedral prifm cannot lead to the integrant particle: therefore all fections, though perfectly practicable in cryftals, will not lead to the integrant particle.

made

Explanation of the figures and formation of cryftals by the method of Hauy.

made on each of thofe fubftançes, and immediately compare them together; when with pleafure he found that there was not the leaft variation between them. Thus had the Abbé Hauy difcovered by the help of geometry alone, that which was confirmed by chemical analyfis, and this fatisfactory accord between two fciences apparently fo diftant from each other, while fecuring each others fteps, ferves alfo to fhew the certainty of the principles on which they are grounded."

Journal des Mines, xxxvii. page 21.

A more fingular anecdote is what took place with refpect to the emerald and the beril. Vauquelin had analyfed the emerald of Prerou, and read the refult of it, at a fitting of the *Ecole des Mines*, which is preferved in the Journal, No. XXXVIII. page 96: viz. of filex 64, of alumina 29, oxide of chroma 3, of lime one, and of volatile fubftance 2; I have neglected decimals. Soon after he difcovered a new earth, which he called the Glucine, and gives the following account of it to the National Inftitute: "The Abbé Hauy having obferved a perfect conformity between the ftructure, the hardnefs, and the weight of the beril and the emerald, preffed me a few months back to make an analyfis of thefe two fubftances, to know whether they contained the fame principles, and in fimilar proportions. In the refult, the fact that will moft intereft the Inftitute is the difcovery of a new earth, &c. &c." *Annales de Chimie*, vol. XXVI. p. 157.

I am certain, Sir, that it will give you no lefs pleafure to learn that Vauquelin, in confequence of this difcovery, made an *addition to the paper* read at the *Ecoles des Mines*, beginning thus, "Since the reading of the above paper, having difcovered a new earth in the beril, and as this ftone, according to the obfervations made by Hauy, contained fubftances fimilar to the emerald, I have in that point of view made a new analyfis of this latter ftone." And the former analyfis was immediately corrected, and the 29 parts of alumina became 16 parts of alumina and 13 *of a new earth*. I hope, Sir, it is not too much to fay that, on this occafion a new earth was difcovered, if not by, at leaft in confequence of, a geometrical analyfis.

But to return to our fubject, the Abbé makes a diftinction between the *integrant particle* and the *primitive form*. The former, as I have faid, is that laft particle, which preferving an exact proportion of the component parts, contains the *leaft*

number

number of thofe parts; it is the laft term of mineralogical analyfis. The primitive form, on the contrary, is its firft refult, and retaining the exact proportion of the component parts, contains the *greateft number* of thofe parts. It is eafy to fee that in the cafe above mentioned of the phofphate of lime, the hexahedral prifm will be the primitive form, precifely for the reafons adduced to fhew that it is not the integrant particle. Though the Abbé does not decidedly define the integrant particle as containing the *minimum* of fpace under the *maximum* of furface, and the primitive form as containing the *maximum* of fpace under the *minimum* of furface,* neverthelefs he makes a remark that authorifes the above definitions (which, Sir, you will obferve are mine, left any fault be found with them.) He fays the dodecahedron with rhomboidal fides, which is the primitive form of the garnet (grenat) contains the *maximum* of fpace under the *minimum* of furface; and if it be cut into two equal and fimilar parts, it will prefent the fame form as the bottom of the cell of the honey-comb, which has the fimilar property.

Explanation of the figures and formation of cryftals by the method of Hauy.

An objection might be taken on the *cuivre pyriteux*, and the *cuivre gris*, or the yellow and grey copper ore of Kirwan, the Abbé mentioning the regular tetrahedron as their primitive form, and not the octahedron as in other cafes. The reafon may be, that all the cryftalline forms of their fubftances which he defcribes are flight modifications of the regular tetrahedron.

"The primitive forms hitherto obferved, fays the Abbé Hauy, are reduced to fix.—The parallelipipedon, the octahedron, the tetrahedron, the regular hexahedral prifm, the dodecahedron bounded by rhombs all equal and fimilar, and the dodecahedron with triangular fides, formed by two right pyramids united bafe to bafe."

He alfo makes a diftinction between integrant particles and *fubftractive particles;* thefe latter are always parallelipipedons, I fhall fpeedily mention whence they derive their name. They are fubftituted for the integrant particles, to facilitate calculations, and it is worthy of obfervation that the parallelipipedon can always be obtained in all diffections of cryftals. Thus far, Sir, I have ftated the firft principles of mineralogical analyfis; I fhall now proceed to the fynthefis.

(To be continued.)

* The inclination of the interfecting planes being the fame.

Defcription

VIII.

Deſcription of a Filtering Machine invented by Profeſſor Parrot of Paris. In a Letter from a Correſpondent.

To Mr. NICHOLSON.

SIR,

Filtering machine by ſand in a reverſed tube.

I ENCLOSE you a drawing of a filtering machine invented by Profeſſor Parrot, of Paris, which for ſimplicity and utility ſeems ſuperior to any other I have met with. You will perceive that from the curvature of its form, it purifies the water both by deſcent and aſcent, and is, conſequently, a cloſer imitation of the operations of nature than thoſe in which the water penetrates in but one direction. Among the advantages which he aſcribes to it, he inſtances the "prolongation of the ſtratum of ſand, which does not conſiderably diminiſh the product of the filtre, but contributes remarkably to the purity of the fluid," and that "the difference of its water-level has an eſſential influence on the quantity of purified water obtained in a given time;" he therefore recommends an apparatus of eighteen inches long from A to D, two inches thick, and four broad, which, he ſays, will yield ſix (Paris) pints of pure water every hour: a machine of this ſize requires only a difference of two or three inches in the height of the water.

I am, Sir,

Your conſtant reader,

London, Auguſt 20, 1804. A. A.

Deſcription of the Machine.

The reſervoir G. (*Fig.* 1, *Plate* IV.) may be of any form or dimenſion which is convenient; the principal part of the machinery conſiſting of a ſquare veſſel bent in the form of an inverted ſyphon. The curve may be circular, elliptic, or in any other direction. This veſſel is to be filled with fine pure ſand to nearly the height of the dotted line *x y*, which denotes the aſcent of the water to D, whence it flows into the receiver. To the part marked A B, which muſt always be above this line, a woollen bag is attached, open at the top and reaching to the ſand: this collects the coarſeſt impurities, and prevents the ſand from becoming foul for a longer time. In large machines

chines a water-tight trap-door may be made at F, for the purpose of removing the ſand when it is overcharged with impurities. The ſmall diameter of the machine from which the drawing was taken, was eight inches from B to E: the perpendicular height from C to A B was eight inches and three-fourths, and from C to D four inches and one-twelfth.

IX.

Medico-Chemical Reſearches on the Virtues and Principles of Cantharides. By H. BEAUPOIL.

(Concluded from Page 71 of Vol. VIII.)

Experiments and obſervations on cantharides.

HE alſo proved that the black precipitate eaſily became dry, brittle, and friable in the air; that it reddened the tincture of turnſole; that it combined very readily with potaſh, diſengaging ammonia; that, when diſtilled by an open fire, it ſwelled and yielded an acid liquor, a thick oil, and carbonate of ammonia; and that it left a dry, ſhining, friable coal in the retort.

Proceeding afterwards to the examination of the yellow matter remaining in ſolution in the alcohol, Cit. Beaupoil informs us, that, when it is concentrated by the evaporation of its ſolvent, it retains the ſame odour and the ſame taſte as the extract; that it is completely diſſolved in water, and reddens the tincture of turnſole; that it combines entirely with potaſh, without any diſengagement of ammonia; and that the reſult of this combination is an homogeneous and glutinous body, ſoluble in water and precipitable by a weak acid; finally, that, diſtilled by an open fire, it ſwells very little, yields an acid liquor, a black and fetid oil, and carbonate of ammonia; but that, in general, all theſe products are in ſmaller quantity than in thoſe obtained from the black precipitate.

Among theſe different reſults the author thought it neceſſary to direct his attention more particularly to the acid, which, as has been ſeen, manifeſts itſelf ſo readily in the infuſion of cantharides, or in the extract which they afford.

At firſt he was of opinion that this acid was analogous to that of vinegar; he alſo thought that its exiſtence might be attributed

Experiments and obſervations on cantharides.

buted to the cuſtom prevalent with thoſe who collect the cantharides, of expoſing them to the vapour of this acid; but when he found, on ſubjecting ſome of theſe inſects which had been procured without the aſſiſtance of vinegar to experiment, that they were ſimilar to thoſe of commerce, he was obliged to renounce his firſt idea, and to endeavour to aſcertain the nature of the acid they offered him, by experiment. It appears that his endeavours have, in this inſtance, been unſucceſsful; for he finiſhes by inferring that his progreſs is not ſufficiently advanced to determine with certainty; and that, although the acid in queſtion has ſome analogy with the phoſphoric, he, nevertheleſs, does not think that it poſſeſſes all its properties, and, conſequently, is of opinion it ſhould be conſidered as a peculiar ſpecies, until new experiments have ſhewn that to which it in reality belongs.

The third product of cantharides, called by Thouvenel and by Cit. Beaupoil, green matter, does not ſeem to experience any change from the air, at leaſt in its phyſical properties. It is inſoluble in cold water; it liquefies in warm water, floating on its ſurface like an oil; alcohol and ether diſſolve it, and its ſolution in theſe two menſtrua is decompoſed by water. Oxigenated muriatic acid brought into contact with this matter, and renewed from time to time, at firſt ſeems not to have any action on it, but at length ſmall whitiſh, brilliant particles are detached from it, which fall to the bottom of the veſſel: in leſs than a week it loſes its ſmell and its colour, and becomes thick and glutinous; and notwithſtanding repeated waſhings, it conſtantly retains the odour of the oxigenated muriatic acid.

Diluted nitric acid, aſſiſted by heat, gives it a ruddy colour, a rancid, penetrating odour, and alſo a conſiderable conſiſtence.

Cauſtic ſoda combines with it without the aid of heat, and without a diſengagement of ammonia. The product of this union is decompoſed by the acids.

Expoſed to an elevated heat it fuſes, and forms a liquid of an oily appearance and ſlightly tranſparent, which, by cooling, quickly reſumes the ſolid ſtate. By a more powerful heat it is decompoſed, its colour changes, a yellowiſh oil, very analogous to that obtained from the diſtillation of wax, and an acid phlegm, paſs into the receivers, but not an atom of carbonate of ammonia.

With

With respect to the parenchyma forming the residue of the different macerations, infusions, and decoctions in water, alcohol, and ether, the author, after having ascertained that these fluids were incapable of extracting any thing more, treated it with caustic potash, which immediately caused a disengagement of a very sensible ammoniacal odour. When this odour was dissipated, the liquor was filtered, and instantly mixed with muriatic acid: the mixture became turbid, and gradually yielded a precipitate, which, dried and thrown on burning coals, exhaled an odour similar to that of animal matters in combustion.

Experiments and observations on cantharides.

Distilled in a retort, this parenchyma yielded phlegm, a dense empyreumatic oil, and a considerable quantity of carbonate of ammonia. The residue of the distillation was a species of coal, from which a white ash was obtained by incineration in the open air, in which were found carbonate of lime, calcareous phosphate, sulphate and muriate of lime, and, finally, oxide of iron.

In recapitulating the quantities of each of the products obtained by means of the experiments which have been cited, the author asserts that one ounce of cantharides, well dried, contains nearly

Black matter	-	1 gros 2 grains;
Yellow matter	-	1 — 2
Green matter	-	1 — 8
Parenchyma	-	4 — 36
Acid	- -	An indeterminate quantity;
Calcareous phosphate		12 grains;
Carbonate of lime	-	2
Sulphate and muriate of lime		4
Oxide of iron	-	2

To complete the work which Cit. Beaupoil had undertaken, it remained for him to determine the physiological properties of cantharides, as well as those of the most essential of their immediate materials, and he appears to have executed this with success in the fourth part of his dissertation. Among other things, it results from the different experiments which he has made on this subject,

1st, That cantharides which have not undergone any preparation, almost always produce disagreeable effects when taken

Experiments and obſervations on cantharides.

taken internally; but that theſe effects, with reſpect to their intenſity, are proportionate to the age, the ſtrength, and the conſtitution of the animals, and to the doſe which has been adminiſtered to them: That the œſophagus, the ſtomach, and the ſmaller inteſtines, are the parts which are principally affected: that thoſe animals which are not overcome by it, experience a deſire to vomit, very conſiderable pains and various affections, which ſeem plainly to indicate that the parts which have been touched by the cantharides, have a ſort of tendency to be diſorganized.

2d. That the aqueous extract of cantharides, in ſmaller doſes than the inſects themſelves, produces nearly the ſame effects as they do; and alſo, that its action on the urinary paſſages is very marked.

3d. That the black matter is much leſs active than the extract; that the animals to which it has been given, are only affected by gripings and vomitings, and very rarely are killed by it.

4th. That the green matter given internally does not appear to have deleterious qualities, ſince all the animals to which even ſtrong doſes had been adminiſtered, did not ſeem to be affected uneaſily by it.

5th. That the yellow matter does not ſeem to be more active than the green matter.

6th. That the extract, the yellow matter, and the black matter, applied ſeparately to the ſurface of the body, occaſioned veſication in nearly the ſame ſpace of time.

7th. That the green matter, applied externally, does not ſeem to act when alone; but that its action is ſpeedily manifeſted when it is divided by wax, and by that means receives the conſiſtence of a cerate.

I muſt not omit to mention, that Cit. Beaupoil was not ſatisfied with experiments made on animals, but had the courage to repeat them on himſelf. It was from having obtained the information he ſought in this manner, that he conſidered himſelf intitled to conclude, that the veſicating property reſides particularly in the extractive part and in the green part of the cantharides, but that the extractive part alone acts on the urinary and genital ſyſtem.

It will be obvious from the details I have given, that the author has carried the examination of cantharides farther than Thouvenel,

Thouvenel. But although his work is greatly extended, it neverthelefs is not yet complete, fince much remains to be done, particularly with refpect to the green matter; for it is difficult to conceive how it fhould have no action on the animal economy when adminiftered internally, fince, applied externally, it produces a vesicating effect. This objection which I have made to the author, the importance of which he acknowledges, will doubtlefs be one of the motives which will determine him to renew his experiments to remove doubts, and to fhew more clearly what is to be expected from the employment of the different parts compofing an ingredient from which medicine has received fuch great benefits.

Experiments and obfervations on cantharides.

X.

Experimental Determinations of the Latent Heat of Spermaceti, Bees' Wax, Tin, Bifmuth, Lead, Zinc, and Sulphur. By Mr. William Irvine. *Communicated by the Author.*

Bedford Street, Aug. 24, 1804.

It will fcarcely be denied that the difcovery of the exiftence of latent heat in all fluid and vaporous bodies, is one of the moft curious and important hitherto developed in the progrefs of chemical philofophy. The merit of firft inveftigating this fubject is univerfally attributed to the celebrated Black. By a few fimple and clear experiments he demonftrated, that, before any portion of ice can become water, it muft receive or abforb as much heat as would have raifed the temperature of an equal quantity of water by 140°. By other experiments, in fome of which Dr. Black was affifted by my father and Mr. Watt, it was proved in a manner equally fatisfactory, that water cannot be converted into fteam unlefs it admit a quantity of heat fufficient to have heated the water 8 or 900°. Having proceeded fo far by experiment, Dr. Black made a general inference, and extended his theory to all cafes of fufion and vaporization whatever.

Difcovery of latent heat by Dr. Black.

The only other philofopher, as far as I know, who has attempted to determine the exact quantity of the latent heat in other bodies befides water, was Dr. Irvine. Landriani made fome

Dr. Irvine's examination of the latent heat of other bodies befides water.

ſome experiments to prove that the fluidity of alum, ſulphur, and ſome metals, was accompanied with an abſorption of latent heat; but I believe he made no attempts to aſcertain the preciſe quantity. In Dr. Black's lectures we are informed, that Dr. Irvine found the latent heat of ſpermaceti to be 145°, of bees' wax to be 175°, and of tin to be 500°. From the very imperfect notes which I poſſeſs of the methods uſed to determine the two former, I believe that the 145° are meaſured by the capacity of fluid ſpermaceti, and the 175° by that of fluid wax: But of this ſubject I will take another opportunity to treat more amply. I am perfectly ignorant of every circumſtance regarding the latent heat of tin as determined by my father, having been unable to find any notes of his experiments for determining this point.

Spermaceti 145°, bees' wax 175°, tin 500°.

During the courſe of the preſent year I have turned my attention a little to this ſubject, I mean to the inveſtigation of the quantity of latent heat neceſſary for the fuſion of various bodies. I was perſuaded that an addition to our knowledge of latent heat would at leaſt increaſe the ſtore of facts, and might perhaps give riſe to ſome improvement or correction of theory.

Experiments made by the author.

The veſſels which I employed in all my experiments were Florence flaſks, of which the neck was cut off. In theſe the water made uſe of was contained, and the veſſel was ſupported on a ſlight wooden ſtand, which preſented a very ſmall ſurface to abſtract heat from the materials. The orifice at the ſuperior part of the veſſel, was in general not more than ſufficient to permit the ready introduction of the fluid examined; probably from an inch and a quarter in diameter to a little more, ſo that a very ſmall ſurface of the water was expoſed to the air. The weight of the glaſs was in all caſes previouſly aſcertained. Theſe circumſtances being premiſed, I proceeded as follows:

The veſſels.

The firſt ſubſtance which I ſubmitted to examination was biſmuth. The melting point of this metal was, by the thermometer which I uſed, 480°. This is ſo near the point found by my father, to wit 476°, that I ſhall conſider his determination as correct, as he combined and compared the different ways of computation and obſervation, and I know the thermometer which he uſed to have been made with conſiderable care. Into a glaſs veſſel which weighed 411 grains, I put 2236 grains of water, of the temperature of 62°. I then removed from the fire a quantity of fluid biſmuth. I waited till it

Determination of the latent heat of fluid biſmuth (or the number of degrees through which any quantity of biſmuth would, by the heat it gives out in congelation, raiſe an equal quantity of biſmuth, from beneath the freezing point of the metal).

it was partially folid, at which time I inferred that both the folid and fluid metal were of the temperature of 476°. I next poured a portion of the fluid bifmuth into the water. In one minute the thermometer ftood at 86°; in two minutes at $85\frac{3}{4}$: The true temperature at the moment of mixture was therefore $86\frac{1}{4}$, fuppofing equal temperatures to be loft in equal intervals. There was alfo a quantity of fteam formed. I weighed the veffel with its contents, and found that it had gained 1589 grains. This therefore fhould be the quantity of bifmuth poured in, if there had been no lofs. But on drying and weighing the metal, it appeared to amount only to 1555 grains, and 34 grains confequently were loft. The bifmuth was cooled $389\frac{3}{4}$°, the water was heated $24\frac{1}{4}$°. Then 1555 grains of water would have gained 34°. Thefe 34°, meafured by the fpecific heat of bifmuth, as ftated in Thomfon's Syftem of Chemiftry at .042, are equal to 810°.9: But the bifmuth, after becoming folid, loft 389°.75, which being fubtracted from 810.9, there remains 421°.15, which cannot be accounted for by the cooling of the folid bifmuth, and muft therefore be the whole or a part of the latent heat of the fufed metal.

Bifmuth, on the point of congealing, was poured into water, the quantities, temperatures, &c. being known; and the common temperature was taken.

Inference of the latent heat.

But the latent heat muft be greater than this, for 411 grains of glafs were alfo heated 24°.25. If the capacity of this glafs be taken at .174, as Kirwan found flint glafs to have, and I have found green bottle glafs to have a capacity of .173 by feveral experiments, whence it is probable that the glafs of Florence flafks has its capacity not very wide of thefe numbers; if then .174 be taken as the capacity of this glafs, thefe 24°.25, which the bifmuth communicated to the 411 grains of glafs in the veffel, are, when meafured by the capacity of bifmuth, equal to 96°.4: And 1555 grains of glafs would have gained 25°.4, which muft be added to 421°.15 already found, and makes 446°.55 for the latent heat of bifmuth.

Correction, for the veffel;

But this is obvioufly ftill too little; for, as has been already mentioned, there was a good deal of fteam formed. The amount of the heat thus loft is extremely difficult to affign. I fhall, however, make an attempt to guefs at, rather than determine it. All the 34 grains muft not be reckoned to have been loft by evaporation. In fpite of all my efforts, I could perceive that fome, though certainly a fmall quantity of bifmuth went off with the water, in form of a number of fmall particles floating in the liquid: and perhaps alfo a fmall por-

and for fteam produced.

tion

tion of water might be wafted during the procefs. If we allow a half of all the lofs to be accounted for in thefe ways, there remain fixteen grains of water which have been converted into fteam. The latent heat of fteam was computed, by Mr. Watt, to be equal to 940°: But this fteam cannot be allowed fo much heat as this; and though it may be difficult to point the quantity to be fairly granted, yet I fhall expect to be within bounds when I eftimate it at one half of 940°, or 470°. In this cafe we fay, 16 grains of water have been heated 470°; fixteen grains of bifmuth would be heated by the fame quantity of caloric, 11190°.4; and 1555 grains + 16 grains fuppofed to have gone off with the water, = 1571 grains of bifmuth, would be heated 113°.9. This quantity of heat, therefore, ought to be added to the 446°.55 already found, and would amount, in all, to 560.45 latent heat of bifmuth.

Latent heat of bifmuth deduced 560¼ degrees.

Other experiments with bifmuth.

I am fenfible that there are feveral gratuitous fuppofitions in this laft part of the reafoning, and I do not therefore lay much ftrefs on it. I endeavoured to confirm or refute the truth of the inductions, by making an experiment exactly on the fame principles, but where, by dexterity, I might prevent the formation of fteam wholly or in part. In one inftance I fucceeded tolerably well, and then the latent heat, with every correction, amounted to nearly 600°.

I repeated thefe experiments for determining the latent heat of bifmuth many times, and the refult is expreffed in the following

TABLE:

Tabulated.

No. of Experiments.	Latent Heat by firft Computation.	Correction for the Heat received by Glafs.	Whole Latent Heat.
1	457	23	480
2	411	29	440
3	412	28	440
4	465	33	498
5	480	29	509
6	438	27	465
7	465	30	495
Mean	446.8		475

On

On the whole, therefore, it appears that we ſhall not exceed the truth when we eſtimate the latent heat of biſmuth at 550°. Mean reſult 550° for the latent heat of biſmuth.

I made, in like manner, two experiments to aſcertain the latent heat of tin, and of theſe the reſults were— Experiments on tin. Latent heat 507°.

Expt.		
1	520	Mean, 507°.
2	495	

This agrees remarkably well with the determination of this point, ſaid to have been made by my father, in Black's Lectures. Latent heat of zinc 493°.

Zinc is computed, by Bergman, to melt at 700° of Fahrenheit's thermometer. Taking that for granted, I made three experiments on the latent heat of zinc, in a ſimilar way with that already related with biſmuth. The reſults were as follow:

Experiment.	Latent Heat by firſt Computation.	Corrected.	Whole L. H.
1	490	28	518
2	476	22	498
3	443	32	475
Mean	469		493

In theſe experiments no allowance is made for loſs by ſteam, which, however, by the dexterity acquired by practice, I was enabled to render very ſmall. Latent heat of lead 146°

Lead I found to melt at a point above 584°. Owing to the ſhortneſs of the thermometric ſcale, I could obſerve no higher. I ſuppoſe therefore that 594°, as found by Dr. Irvine, cannot be materially diſtant from the truth. Aſſuming it as true, I proceeded to make ſeveral experiments on the latent heat of lead. In doing this I was led immediately to notice, that melted lead does not by any means produce ſo much ſteam as other metals do when poured into water, even under the moſt careleſs management; and of this the reaſon will appear from the following table:

Experiments on the Latent Heat of Lead.

No.	Latent Heat.	Corrected.	Whole Latent Heat.
1	127.8	10°.3	138°.1
2	142.8	11°.	153°.8
3	149.9		
4	161.8		
5	131.5		
Mean	142.7	Mean	145°.9

In these experiments I have, by accident, lost the notes of the determination of the weight of the vessel employed in all but the two first instances: notwithstanding which it may be fairly inferred, that the mean latent heat of lead is about 150°, a quantity certainly unexpectedly small, and which, in many more experiments than these related, I was at pains to examine, without being able to discover any material inaccuracy. In the second experiment, where a little steam was formed, I ascertained the loss to be four grains. If these four grains be supposed, as in the case of bismuth, to contain 470° of heat, the computed addition to the latent heat of lead will be 20°.3, making in all a little more than 162°. This is certainly a very peculiar and unexpected quality of this metal.

Experiments on sulphur. The only other substance which I have submitted to examination for the purpose of ascertaining its caloric of fluidity, is sulphur. The melting point of sulphur is commonly stated, in elementary works, to be at 212°: But that this is not accurate any one may convince himself, by immersing a quantity of sulphur in boiling water, where it remains altogether unaltered. By every trial which I have been able to make, I am convinced that the fusion of sulphur takes place about the temperature of 226°. I say about, because the communication of heat among the particles of sulphur is very slow, and the thermometer is often encrusted with solid sulphur, which, some how or other, certainly cools below the liquid in which it is immersed. In experiments for ascertaining this point, the thermometer ought to be kept in constant motion. The results of my experiments for finding the latent heat of sulphur, are stated in the following table.

Experiments

Experiments to ascertain the Latent Heat of Sulphur.

Latent heat of sulphur 143½°.

No.	Latent Heat.	Corrected.	Whole Latent Heat.
1	144°.56	8°.	152°.56
2	131°	7°.	138°.
3	140°	4°.5	140°. 5
4	136°		
Mean	137°.89	Mean	143°.68

In these experiments I have supposed, from experiments of my own, the capacity of sulphur to be .189, which does not materially differ from Mr. Kirwan's determination. In the other cases I have trusted chiefly to the numbers given in Thomson's Chemistry, though these are not always wholly unexceptionable. In every instance I have supposed the semiliquid to have the temperature of the melting point, which I believe is generally true; but some practice is required to seize the moment before the frozen particles float in the fluid substance. In experiments on sulphur especially, inattention to this circumstance causes very great inaccuracy, and was the cause of considerable embarrassment to me before I observed my error.

A comparative table follows of the caloric of fluidity of all substances hitherto examined:

Table of latent heats.

Substance.	Melting Point.	Latent Heat.	Ditto in Degrees measured by Capacity of Water.
Ice	32°	155°.555	140°
Spermaceti	113°	145°	
Bees' Wax	142°	175°	
Tin	442°	500°	33°
Bismuth	476°	550°	23°.65
Lead	594°	162°	5°.604
Zinc	700°	493°	48°.3
Sulphur	226°	143°.68	27°.145

General remarks.

In all these instances the latent heat is expressed in degrees measured by the capacity of the relative solid, excepting in the cases of spermaceti and bees' wax, which are in degrees measured by the capacity of the fluid. I endeavoured to rec-

tify this fo as to make the comparifon more fair, by determining the fpecific heat of folid wax and fpermaceti; but I have not been able to fatisfy myfelf with either of thefe points, owing to the foftnefs and confequent abforption of latent heat, which a very low degree of heat induces in both thefe bodies. The numbers expreffing their latent heats are therefore too low. On infpection of the above table, there does not appear any ratio by which the quantity of the caloric of fluidity feems to be guided: it certainly does not increafe with the difficulty of fufion, but moft probably has fome connection with the relative capacity of each body in its folid and fluid ftate. The determination, however, of the capacity of any of the metals in a fluid form, excepting mercury, muft be regarded as an extremely difficult tafk.

XI.

Strictures on Mr. DALTON'S *Doctrine of Mixed Gafes, and an Anfwer to Mr.* HENRY'S *Defence of the fame. In a letter from Mr.* JOHN GOUGH.

To Mr. NICHOLSON.

SIR,

Caufes of thefe ftrictures.

I HAVE ventured to defend the chemical union of water and air, as well as the homogenity of the atmofpherical gas. My thoughts on thefe fubjects are briefly ftated in your Journal *; and the farther profecution of the enquiry compels me to make an open attack on my friend Mr. Dalton, and his new convert, Mr. Henry. The difpute fhall be fairly conducted on my part; that is, it fhall confift of arguments which I am ready to abandon as foon as they are refuted; this promife is due to friendfhip, as well as the obligations of truth.

Dalton's a hypothefis not a theory.

The firft thing to be afcertained is the proper appellation of Mr. Dalton's opinions. His doctrine of mixed gafes is offered to the public as a mechanical theory, founded on chemical facts: a little attention, however, to Mr. Dalton's effays, will deprive it of all claim to the title of a theory. This is evident from the nature of the mechanical philofophy; every branch

* Vol. VIII. Page 243.

of

of which admits of a mathematical demonſtration, derived from Newton's definitions and laws of motion: but Mr. Dalton has not attempted to give ſtability to his new ideas, by the aid of the mathematics; on which account the only appellation due to his doctrine, is that of a hypotheſis.

The hypotheſis not mechanical.

No philoſopher ought to diſregard the means which are able to confirm his opinions, becauſe theſe means may be found, upon trial, to ſubvert them. This I believe to be the caſe with my friend's hypotheſis; for I have endeavoured to ſhew the fallacy of it by mathematical arguments. The reſult of this attempt will in all probability appear in the next volume of the Mancheſter Memoirs; when the merits of the eſſay will be determined by geometricians, who are the proper judges of ſuch productions. This want of geometrical demonſtration eſcapes the notice of the chemiſt, becauſe my friend has ſeemingly ſupplied the deficiency by a number of probabilities of an experimental nature; but it is almoſt ſuperfluous to remind either him or your readers, that a myriad of ſuch proofs cannot uphold a doctrine which is repugnant to the mechanical philoſophy.

Concluſions from theſe probabilities.

The two leading maxims which are derived from theſe probabilities, and form the baſis of the hypotheſis, are thus briefly expreſſed by Mr. Henry: "mixed gaſes neither attract nor repel each other, and every gas is as a vacuum to every other gas*". Mr. Dalton, reaſoning from theſe premiſes, ſurrounds our globe with an independent atmoſphere of vapour, the preſſure of which preſerves all the water on the earth's ſurface in a liquid ſtate, and prevents the ocean itſelf from eſcaping through the air, by virtue of its own elaſtic force; at leaſt the laſt is a fair inference from his own concluſions.

Dalton's reaſoning incomplete.

Although I have demonſtrated the exiſtence of an atmoſphere of vapour to be a mechanical impoſſibility, in the eſſay mentioned above, the reality of the thing ſhall be ſuppoſed at preſent, for the purpoſe of detecting the fallacy of the doctrine in the fundamental maxims of it. The truth is, Mr. Dalton has diſcontinued his train of reaſoning too ſoon; for, had his arguments been purſued to their proper limits, they would have diſcovered the incompatibility of the hypotheſis and natural appearances. This omiſſion is eaſily ſupplied, and will be as eaſily comprehended, by a perſon who underſtands the laws of hydroſtatics.

* Phil. Journ. Vol. VIII. Page 298.

If

A second particle may follow a former.

If a particle of vapour can pass freely through the air, a second can also succeed it at any given distance; because the latter may undoubtedly pursue the track which the former has already traversed: consequently, a series of such particles, possessing the density of water, might be raised into each perpendicular pore of the atmosphere, by the application of a proper force to the surface of a collection of water supporting such an atmosphere. Now the existence of such a force is certain; because when air is injected into either leg of an inverted syphon containing water, it constantly disturbs the equilibrium of this fluid; that is, the gas presses upon the liquid, notwithstanding the supposition, that the former is a vacuum to the particles constituting the latter. If now we combine the postulates of the hypothesis with the preceding fact, a column of air, which occupies the upper part of a vessel containing water in its bottom, becomes a heavy piston, having its substance perforated in every direction by pores of easy transmission, which are, at the same time, separated by partitions impervious to water. After contemplating this imaginary structure of the atmospherical gas, let the reader investigate the consequences of it, and compare them with natural events. The impenetrable parts of the gaseous piston would compel the water to ascend along its perpendicular pores; in which it would be kept duly condensed by the pressure of the incumbent vapour. The specific gravity of this column, compounded of air and water, would exceed that of the external air; consequently the upper extremity of it would constantly glide over the edges of the vessel into the atmosphere. Thus there would happen a double loss of water, namely, by evaporation and percolation: but nothing of the kind is observable in nature; consequently the air is impenetrable to the constituent particles of water not heated to the boiling point.

Air presses upon water.

Air a perforated piston, by the hypothesis.

Air not a vacuum to vapour.

Impenetrability no obstacle to chemical union.

The circumstance of water not being able to penetrate air at low temperatures, is no obstacle to the chemical union of the two substances; for many aqueous solutions of salts occupy less space, when completed, than the materials formerly did of which they are composed. This fact has been established by the present Bishop of Llandaff; and it proves that bodies, which are mutually impenetrable, may be susceptible of the bond of chemical affinity.

One gas not a vacuum to another.

After

After all that has been ſaid about the conſtitution of gaſes, the notion is incorrect, ſtrictly ſpeaking, which ſuppoſes each gas to be as a vacuum to every other. The foregoing obſervation may alſo be extended to mixtures of carbonic acid gas with water; and the following inſtances may be adduced in confirmation of the propoſition: When a particular gas is developed in a veſſel containing common air, the firſt portion that comes over has in it leſs of the ſpecific gas than the ſecond; nor is the ſecond equal to the third in purity. This circumſtance ſhews, that the new-formed gas does not find a vacuum in the air of the veſſel; on the contrary, the two fluids produce a mechanical mixture, which is gradually expelled, until no part of the air remains in the tube or bottle. In like manner, if a bottle of highly ærated water be opened ſuddenly, the rapid expanſion of the liberated gas ejects a great part of the contents, thereby proving that water oppoſes an obſtacle to the dilatation of this gas. In reality, Mr. Dalton allows one gas to be an impediment to the motions of another; but at the ſame time he maintains, that two ſuch fluids finally overcome their mutual obſtructions, and occupy the ſame ſpace in a ſtate of perfect independence. This is a propoſition which may be juſtly ſuſpected of being a ſoleciſm in pneumatics, until the author of it has proved the contrary by a rigorous demonſtration.

Proved by experiment.

Amongſt the many probabilities which have been offered in ſupport of the hypotheſis, perhaps none are more ingenious than the remarks of Mr. Henry; and the part which I have taken in the preſent letter, obliges me to place them in a new light. This attempt muſt, however, be preceded by a theory, which will explain the relations of thoſe gaſes that neither attract nor repel each other: ſuch an explanation became a neceſſary part of pneumatics, from the time when Dr. Prieſtley made his experiments upon mixtures of this deſcription. The following, then, is a ſketch of a theory, having for its foundations the mutual repulſion of homogeneous particles, and the reciprocal reſiſtance which gaſes have been ſhewn to poſſeſs.

A new theory of mixed gaſes neceſſary.

When two ſuch gaſes come into contact, parcels of each will be detached by every ſlight force, and enveloped in the ſubſtance of the other. In this manner, the two fluids will be broken to pieces, and blended in one maſs forming a mechanical

Sketch of the theory.

nical mixture; the component parts of which cannot be ſeparated without the intervention of chemical agents, becauſe the disjoined fragments of each gas will be prevented from re-

Exception to the general rule.

uniting by their mutual repulſion. There is one exception, however, to the general rule; for when a liquid is found in a mixture, the component parts will follow the law of their ſpecific gravities; becauſe an inelaſtic fluid is not actuated by an intrinſic repulſion, in all other caſes the gaſeous fragments will continue to be farther and farther ſubdivided, and will conſtantly aſſume a new arrangement from the ſligheſt agitations. The conſtituent parts of ſuch a mixture exert their force in perfect union; and this circumſtance diſtinguiſhes it from Mr. Dalton's compound, the conſtituent gaſes of which preſs ſeparately upon all ſurfaces.

The theory applied to Mr. Henry's experiments.

The neceſſary premiſes being now ſettled, it is time to try the powers of the theory upon Mr. Henry's experiments. If, then, ten meaſures of water, containing an equal bulk of carbonic acid gas, be preſſed by a column of the ſame gas, equivalent in force to 30 inches of mercury, the ſtate of the aqueous compound will remain invariable; becauſe the ſpring of the gas in the water appears, by Mr. Henry's experiments, to be equal to the ſpring of the incumbent gas: therefore, ſhould a gaſeous particle happen to eſcape from the compound, an equal particle, from the upper part of the veſſel, will replace it immediately. But if the incumbent carbonic acid be made to give place to a maſs of common air of the ſame elaſtic force, the ſurface of the aqueous compound will undergo no change of preſſure; but the gaſeous part of it, meeting with no repulſion from the column of air, will begin to form a mechanical mixture with it, according to the theory. The parcels of the carbonic acid will alſo remain diſtinct, according to the ſame, after their eſcape from the water; and as a number of them will be arranged on the ſurface dividing the two mediums, they will form ſo many obſtacles to the diſcharge of their kindred gas; the egreſs of which will be confined to the intermediate compartments of eaſier tranſmiſſion conſiſting of common air. The diviſion of the ſurface between the two mediums, into compartments of eaſy tranſmiſſion and impenetrable points, ultimately produces a ſort of equilibrium, which aſſigns their reſpective portions of the elaſtic acid, to the gaſe-

ous

ous compounds. The origin of this equilibrium is eafily collected from the preceding theory and the laws of pneumatics. For the globules of carbonic acid, contained in the gafeous compound, invariably exert the force of 30 inches of mercury; they therefore prove too powerful for the rarefied gas of the water, which they comprefs, and infinuate part of their own fubftances into the fluid. Whilft this operation is going on at innumerable points in the furface, portions of the fame gas are conftantly forced through the compartments of eafy tranfmiffion, by the flighteft agitations. Now the quantity of gas which is difcharged into the air of the jar, is greateft at firft, and continually diminifhes; on the contrary, the quantity that is forced into the water through the former paffages, is leaft at firft, and increafes afterwards; confequently the two quantities ultimately become equal, and eftablifh the equilibrium in queftion, by the contrariety of their effects.

Concluding remarks.

The preceding is a general propofition, which explains a variety of appearances, fuch as Mr. Henry has defcribed, by by the well known principles of mechanics. Should the confideration of it be attended with conviction on his part, he perhaps will give a new turn to his experimental enquiries, and endeavour to difcover the law of affinity, which connects water with the different gafes. It is almoft certain, that this law is not the fame in all cafes, as will be feen by comparing the experiments of M. la Sauffure on the folution of water in common air, with Mr. Kirwan's obfervations on the folvent powers of hidrogen; which may be found at page 14 of the firft edition of his Effay on Phlogifton.

JOHN GOUGH.

Middlefhaw, Aug. 23, 1804.

P. S. The following error requires correction: In Vol. VIII. page 244, line 21, before the words, *as oft as water*, read *therefore*, and ftrike out the fame conjunction in line 22.

An

XII.

An Enquiry concerning the Nature of Heat and the Modes of its Communication. By BENJAMIN COUNT *of Rumford, V. P. R. S. &c. Abridged from the Philosophical Transactions for the Year* 1804.

Experiments on heat.

AFTER remarking that all discoveries on an agent of such extended operation as heat cannot fail to be eminently useful, the author proceeds to describe the apparatus used in the experiments now to be described: They were the following.

Instruments. Thermometers.

1. Mercurial thermometers carefully constructed, having cylindrical bulbs, four inches long and four tenths of an inch in diameter, and their tubes from 15 to 16 inches long; the air being excluded, and the graduations according to Fahrenheit, exhibiting eight parts of degrees by means of a nonius.

Instruments. Cylinders to contain hot water, and suffered to cool with different kinds of clothing.

2. Four cylindrical vessels of thin sheet brass, for ascertaining the warmth of clothing. *Fig.* 1. *Plate* I. The vessel is closed at both ends; but has a neck at the top, into which hot water is occasionally poured, and in which one of the thermometers is fitted and placed during the time of an experiment, so that its long bulb shall occupy the axis of the vessel, and will shew its mean temperature. Another cylindrical neck proceeds from the lower surface, and is fitted upon the adjustable part of the wooden stem beneath. The vessels are four inches long and four inches diameter, and the necks are about eight tenths of an inch in diameter, the upper one being four inches long and the lower three inches. When the vessel is clothed and charged with hot water, its rate of cooling by exposure to the quiet air of a large room, will shew the relative warmth of each particular kind of clothing.

In some of the experiments the ends of the instrument were permanently covered by the application of a thin wooden box to each, the box being varnished and covered with fine writing paper, and filled with fine eider down, and a cap of fur was pulled over the box, and the projecting neck. The clothing of these cases was applied for experiments to the cylindrical surface.

Two of the instruments (No. 1 and 2.) were thus covered up at the ends, and the other two (No. 3 and 4) were left in the state represented *Fig.* 1. without the permanent coverings.

In

In each experiment two ſimilar inſtruments (ſuppoſe No. 1 and No. 2, or No. 3 and No. 4) were uſed, one *naked* and the other *covered*; ſo that in each experiment the naked inſtrument ſerved as the ſtandard of compariſon with the other. Method of operating with the cylinders to ſhew the effect of clothing.

The experiments were made and regiſtered in the following manner: the two inſtruments uſed in the experiment, placed over their wooden ſtands, being ſet down on the floor, were filled to within about $1\frac{1}{2}$ inch of the tops of their cylindrical necks with boiling hot water; and a thermometer being put into each of them, they were placed at the diſtance of three feet from each other, on a large table in a corner of a large quiet room, 24 feet long, 19 feet wide and 13 feet high, where they were ſuffered to cool undiſturbed. Near them, on the ſame table, and at the ſame height above the table, there was placed another thermometer, ſuſpended in the air to the arm of a ſtand, to aſcertain the temperature of the air.

Every caution was uſed to prevent diſturbance by currents or agitation of the air, whether by partial heat or the intruſion of any perſon during the progreſs of any experiment.

By the reſults of a great number of experiments, it was found that the ſame inſtrument cooled through any given (ſmall) number of degrees, for inſtance 10°, in very nearly the ſame time, whatever was the temperature of the air of the room; provided always that the point from which theſe 10 degrees commenced, was at ſome conſtant number of degrees above the temperature of the air at the time being. The interval choſen by the Count lay between the 50th and the 40th degrees above the temperature of the air in which the inſtrument was expoſed to cool; when for inſtance, the air was at 58°, the interval commenced at 108, and ended at 98°.— When the air was at $64\frac{1}{2}$° it commenced at $114\frac{1}{2}$° and ended at $104\frac{1}{2}$°. Scale through which the refrigeration was meaſured.

The warmth of any covering, or its power to confine heat, was eſtimated by the time employed in cooling through that interval.

As it ſometimes happened, though very ſeldom, in the courſe of an experiment (which commonly laſted ſeveral hours) that the moment of the paſſage of the mercury through one or both of theſe extreme points was not obſerved, it was of importance to determine the ſame by interpolation from the other points obſerved. To do this, the author availed himſelf of the Method of ſupplying by interpolation the extreme inſtants of time elapſed, when omitted to be obſerved.

the following law of cooling of hot bodies in a fluid, which he found by experiment to be applicable without ſenſible error in the preſent caſe: It is that, if the equal portions of a right line repreſent ſucceſſive intervals of time, and perpendiculars be erected upon the ſame, to denote by their lengths the degrees of the exceſs of temperature of the hot body beyond that of the cold medium, at points denoting the correſponding inſtants of time, the line joining the extremities of the perpendiculars will be the logarithmic curve. Whence if two temperatures and the elapſed time be obſerved, it will be eaſy by the help of a table of logarithms to determine the time at which any intermediate temperature took place. This is exemplied by the Count, who then proceeds to relate his experiments.

Exp. 1. A clothing of Iriſh linen cauſes the heat to eſcape much quicker than from a bright metallic ſurface.

Exp. 1. One of the veſſels, No. 1. having its ends clothed as before deſcribed, and its poliſhed ſides naked, was filled with hot water. Another veſſel, No. 2. alike in all reſpects, but having its ſides cloſely clothed with Iriſh linen, ſuch as is ſold in London at 4*s*. per yard, was filled in like manner, and both were ſimilarly expoſed to cool.

The naked inſtrument employed 55 minutes in cooling, from 94° to 84° Fahrenheit (the air of the room being at 45°.)—But the clothed inſtrument was cooled through the ſame interval in 36½ minutes, conſequently this clothing expedited the emiſſion of heat inſtead of confining it.

When both inſtruments were cooled to 42° they were removed into a warmer room at 62°, and the clothed inſtrument was alſo found to acquire heat conſiderably faſter than the other.

Whether the linen accelerated the cooling by aſſiſting the ſucceſſion of freſh particles of air, or by promoting the eſcape of heat by radiation, were points to be determined.

Exp. 2. The ſame effect from a coating of glue.

Exp. 2. To decide this, the linen of No. 2. was removed, and the ſides were thinly coated with glue. In theſe circumſtances, while the ſtandard or naked inſtrument cooled through the interval in 55 minutes, the coated inſtrument employed only 43¼ minutes.

In reaſoning upon this experiment, the author concluded, that if the glue operated only by preventing the air from attaching and fixing itſelf to the poliſhed metallic ſurface, and conſequently in that manner facilitated its circulation and the cooling; it would be of no conſequence, whether the ſurface

4 was

was covered by one or more coatings of glue: But, on the contrary, if the radiations of heat were facilitated and increafed, it might be expected that a greater effect would be produced by two coatings than by one.

Exp. 3. The experiment was therefore tried, and it was found that the inftrument ufed employed only $37\frac{5}{8}$ minutes in paffing through the interval. Exp. 3. More coatings of glue increafed the effect.

Exp. 4, 5, 6, 7. When the experiment was repeated with clear colourlefs fpirit varnifh, the fame effect was produced, and it was augmented as far as by four coatings. But on proceeding as far as eight coatings, the limit of the greateft effect was found to have been paffed. Exp. 4, 5, 6, 7. Alfo fpirit varnifh,

Exp. 8, 9, 10. Black paint (lamp black and fize) upon the varnifh increafed the cooling effect a little. When the inftrument was cleaned and then painted, its rapidity of cooling was nearly the fame = 35 minutes. And with white paint the difference was not confiderable, as the effect was produced in 36 minutes. As the paint was laid on in feveral fucceffive coatings, the Count remarks that little dependence is to be placed on thefe refults as indicating a difference from colour. Exp. 8, 9, 10. —and black fize paint, —and white,

Exp. 11. The clean inftrument being fmoked black over a wax candle, was found to have cooled through the interval in $36\frac{1}{8}$ minutes, while the ftandard employed $55\frac{7}{8}$ minutes. The lamp black, when wiped off and weighed, amounted to lefs than $\frac{1}{18}$ of a grain, though it had completely covered 50 fquare inches. Exp. 11. And of fmoke from a candle.

With a view to a more accurate determination of the velocity of cooling, it was neceffary to afcertain what heat efcaped through the permanent clothing at the ends. This was done by obferving the times of cooling with and without the clothing, and comparing the effects with the refpective furfaces of expofure. For the whole furface of the inftrument 85.195 fquare inches, is to the furface of its vertical fides; fo is the whole quantity of heat paffed off = 10000 to the quantity that paffed through the fame fides = 5885. And fince by obfervation the naked inftrument required $45\frac{1}{2}$ minutes to cool through the fame interval as was paffed through in $55\frac{1}{2}$ minutes when the ends were covered, the author concludes that $45\frac{1}{2} : 55\frac{1}{2} :: 5885 : 7015 =$ what would have paffed in the later interval through the upright furface, and confequently that the remainder of the heat = 2985 parts muft have paffed through the covered parts of the inftrument. Determination of the efcape of heat at the flat ends of the inftruments.

By

By applying these results to the numbers in *Exp.* 11. where the cooling was effected in $36\frac{1}{8}$ minutes, the Count says as $55\frac{1}{2}$ minutes give 2985 heat passed through the covered ends, so will $36\frac{1}{8}$ minutes give 1942 parts. And this taken from 10000, the whole heat lost, will leave 8058 for the heat that really passed through the upright sides. But it was found that 7015 pass through the naked sides in $55\frac{1}{2}$ minutes: Whence $7015 : 55\frac{1}{2} :: 8058 : 63\frac{3}{4}$. And consequently, the corrected times are $36\frac{1}{8}$ and $63\frac{3}{4}$, which express the velocities of the passage of heat through the surface of the naked metal, and that which was blackened with smoke, viz. as 5654 to 10000 nearly.

Whence the results are corrected.

In the same manner the velocities of the passage of heat in the experiment No. 6. are shewn to have been as 4566 to 10000.

A new course of experiments.

It has been remarked that, in these curious instances of the effects of modification of surface or clothing upon the transitions of heat, the effect may have been favoured by communication to the air, or by facilitating the process of radiation. The author's reasoning upon *Exp.* 2. appeared not so decisive as to need no support from experiments of a different class. He therefore constructed an instrument for measuring the effects of radiation, which is seen in *Fig.* 2, *Plate* I.

Instruments for improving radiant heat.

Like the hygrometer of Mr. Leslie,* (as the Count observes) it consists of two glass balls at the ends of a tube C and E. The tube is of such a diameter that one inch in length would contain 15 grains of mercury; the balls are 1.625 inches in diameter; the upright ends of the tube C and E are each 10 inches long; the horizontal part D is 17 inches; and the board A B, to which it is attached, is 27 inches long, 9 inches wide, and one inch thick. The pillar F supports a circular vertical screen made of pasteboard, covered with gilt paper on both sides, the use of which is to protect one of the balls from rays intended only to act upon the other. The balls contain only air, and a small drop of coloured spirit of wine is introduced by means of a short tube projecting from one of the elbows; which short tube is then hermetically sealed. By a little management the bubble of spirit is brought to rest in the middle of the horizontal tube; and when the temperature of

* Philosophical Journal, quarto series, III. 461.

the

the air in either of the balls is made to exceed that of the other, the increaſed elaſticity cauſes the bubble to move towards the colder ball.

By a ſimple contrivance of ſliding boards, the hot bodies were moved by rack-work and a winch to any diſtance from the thermoſcope without the attention of the obſerver being taken off from the bubble in the tube, and the diſtances were alſo ſhewn by a graduated ſcale and nonius. *Fig.* 3. *Plate* I. ſhews one of theſe bodies. It is a metallic cylinder having its baſe, which is to be preſented to the thermoſcope, vertical, and its neck obliquely placed for the purpoſe of introducing the hot Water, and alſo a thermometer for ſhewing its temperature at any required time.

The experiments and obſervations which conſtitute the remainder of this memoir will be given in our next.

XIII.

Experiments and Calculations relative to phyſical Optics. By THOMAS YOUNG, *M. D. F. R. S. From the Philoſophical Tranſactions for* 1804.

I. EXPERIMENTAL DEMONSTRATION OF THE GENERAL LAW OF THE INTERFERENCE OF LIGHT.

IN making ſome experiments on the fringes of colours accompanying ſhadows, I have found ſo ſimple and ſo demonſtrative a proof of the general law of the interference of two portions of light, which I have already endeavoured to eſtabliſh, that I think it right to lay before the Royal Society, a ſhort ſtatement of the facts which appear to me ſo deciſive. The propoſition on which I mean to inſiſt at preſent, is ſimply this, that fringes of colours are produced by the interference of two portions of light; and I think it will not be denied by the moſt prejudiced, that the aſſertion is proved by the experiments I am about to relate, which may be repeated with great eaſe, whenever the ſun ſhines, and without any other apparatus than is at hand to every one.

General laws of the interference of light proved in the production of fringes of colours.

Exper. 1. I made a ſmall hole in a window-ſhutter, and covered it with a piece of thick paper, which I perforated with a fine

Exp. 1. Fringes bordering the ſhadow of a ſlip of card are pro-

duced by the interference of light from both edges. For interception on one side destroys the effect.

a fine needle. For greater convenience of obfervation, I placed a fmall looking glafs without the window-fhutter, in fuch a pofition as to reflect the fun's light, in a direction nearly horizontal, upon the oppofite wall, and to caufe the cone of diverging light to pafs over a table, on which were feveral little fcreens of card-paper. I brought into the fun-beam a flip of card, about one-thirtieth of an inch in breadth, and obferved its fhadow, either on the wall, or on other cards held at different diftances. Befides the fringes of colours on each fide of the fhadow, the fhadow itfelf was divided by fimilar parallel fringes, of fmaller dimenfions, differing in number, according to the diftance at which the fhadow was obferved, but leaving the middle of the fhadow always white. Now thefe fringes were the joint effects of the portions of light paffing on each fide of the flip of card, and inflected, or rather diffracted, into the fhadow. For, a little fcreen being placed a few inches from the card, fo as to receive either edge of the fhadow on its margin, all the fringes which had before been obferved in the fhadow on the wall immediately difappeared, although the light inflected on the other fide was allowed to retain its courfe, and although this light muft have undergone any modification that the proximity of the other edge of the flip of card might have been capable of occafioning. When the interpofing fcreen was more remote from the narrow card, it was neceffary to plunge it more deeply into the fhadow, in order to extinguifh the parallel lines; for here the light, diffracted from the edge of the object, had entered further into the fhadow, in its way towards the fringes. Nor was it for want of a fufficient intenfity of light, that one of the two portions was incapable of producing the fringes alone; for, when they were both uninterrupted, the lines appeared, even if the intenfity was reduced to one-tenth or one-twentieth.

(To be continued.)

Experiments on heat by Count Rumford.

Fig. 2.

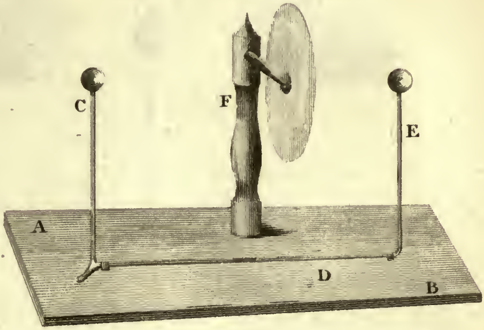


Fig. 1.

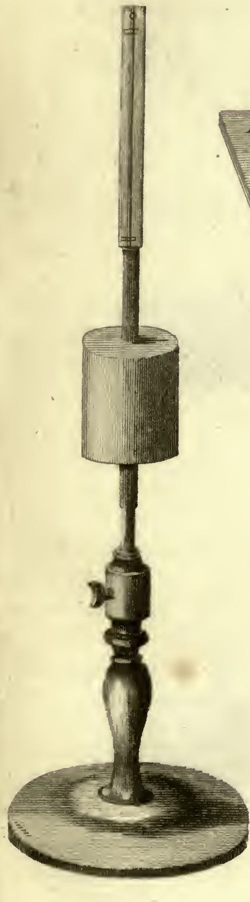
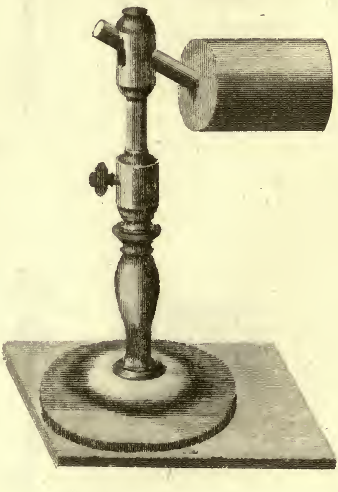


Fig. 3.



Mutlow Sc. Russell Court.

Experiments on heat by Count Rumford.

Fig. 4.

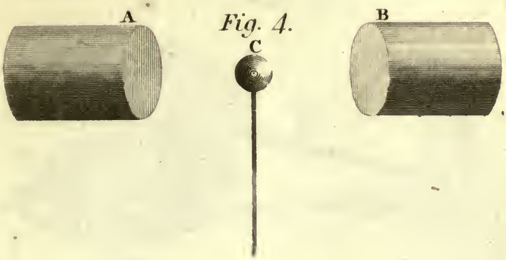


Fig. 5.

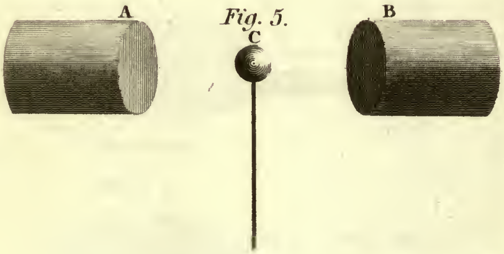
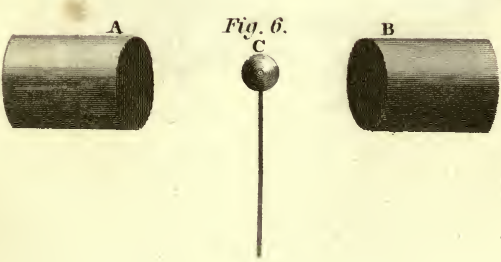
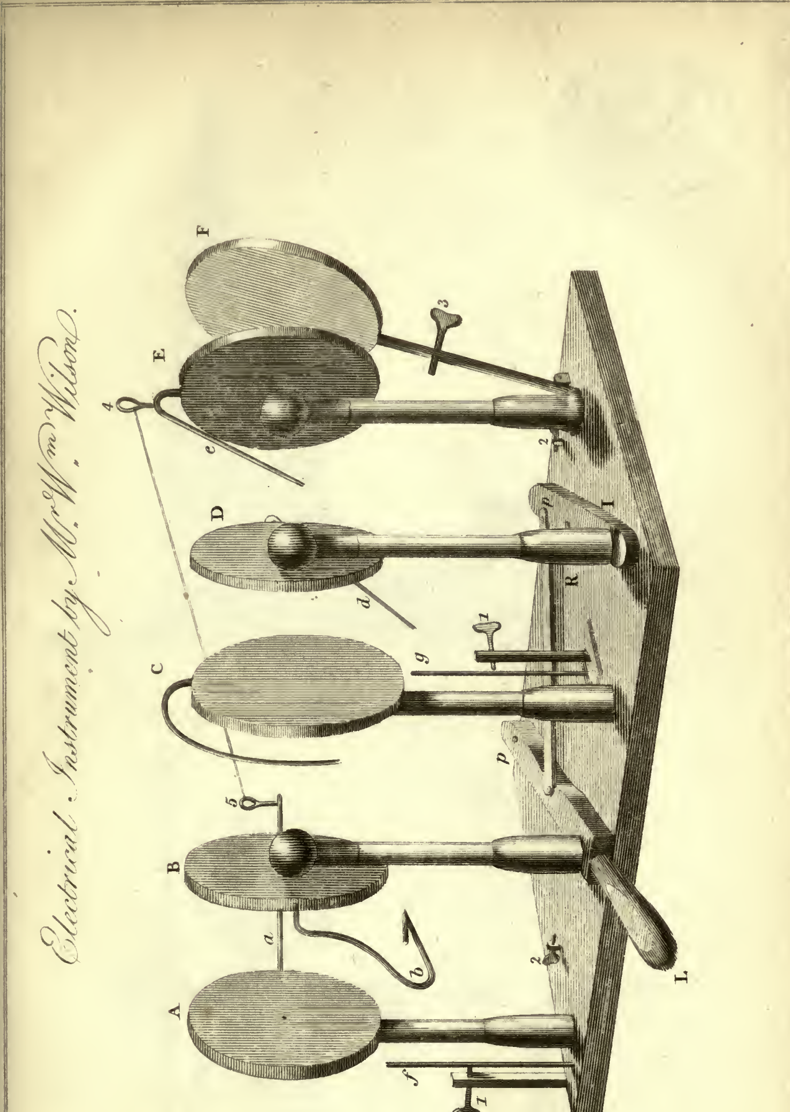


Fig. 6.



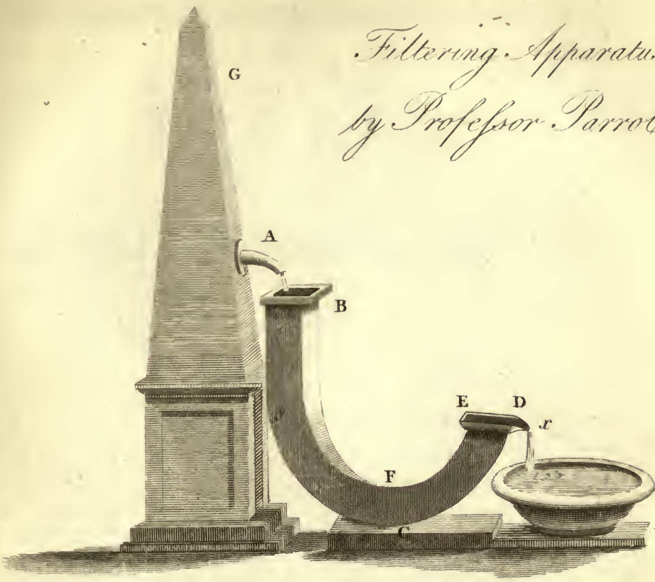
Mutlow Sc. Russell Court.



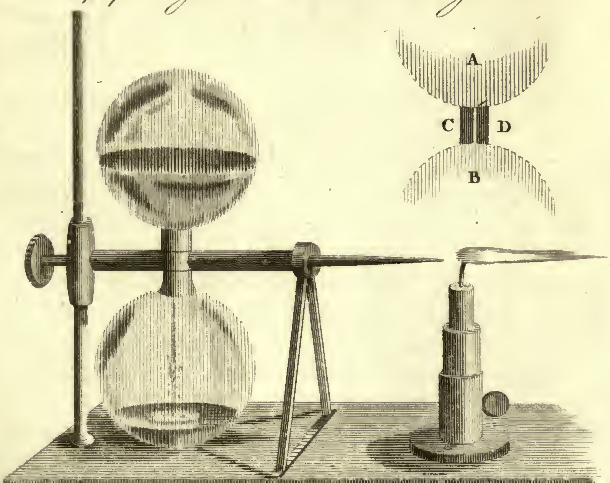
Drawn by Blunt.

Engraved by Mutlow.

Filtering Apparatus, by Professor Parrot.



Blow pipe by the Abbé Melograni.



A JOURNAL OF NATURAL PHILOSOPHY, CHEMISTRY, AND THE ARTS.

OCTOBER, 1804.

ARTICLE I.

Extract of a Letter from Count Apollos de Mouffin Poufhkin, to Charles Hatchett, Efq. F. R. S. defcribing his Method of preparing Malleable Platina. Communicated on the Requeft of the Count, by Charles Hatchett, Efq. F. R. S. *and now firft publifhed.*

1. PRECIPITATE the platina from its folution by muriate of ammonia, and wafh the precipitate with a little cold water. Purif. of platina. 1. Precip. the folution by mur. of amm.

2. Reduce it in a convenient crucible to the well-known fpungy metallic texture, which wafh two or three times with boiling water to carry off any portion of faline matter which may have efcaped the action of the fire. 2. Reduce to the fpongy metal, and wafh.

3. Boil it for about half an hour in as much water mixed with one tenth part of muriatic acid as will cover the mafs to the depth of about half an inch in a convenient glafs veffel. This will carry off any quantity of iron that might ftill exift in the metal. 3. Boil in weak mur. acid, to carry off all iron.

4. Decant the acid water, and edulcorate or ftrongly ignite the platina. 4. Edulcorate and ignite:

5. To one part of this metal take two parts of mercury, and amalgamate in a glafs or porphyry mortar. This amalgamation takes place very readily. The proper method of conducting it is to take about two drams of mercury to three drams of platina, 5. Amalgamate with mercury:

platina, and amalgamate them together; and to this amalgam may be added alternate ſmall quantities of platina and mercury till the whole of the two metals are combined. Several pounds may be thus amalgamated in a few hours, and in the large way a proper mill might ſhorten the operation.

6. Mould the amalgam into pieces; which ſoon become ſolid.

6. After the amalgam is completely produced, it muſt be quickly moulded in bars or plates, or any other forms that may be preferred; taking care that theſe moulded pieces ſhould at leaſt be half an inch in thickneſs, and of a proper length to manage them afterwards in the fire; it is alſo requiſite that the moulds ſhould be perfectly even and ſmooth. Half an hour after the pieces are formed they begin to harden by the oxidation of the mercury, and change their brilliant metallic colour for a dull leaden one.

7. Expel the mercury, by ignition:

Laſtly, Heat the platina ſtrongly, and forge or laminate it.

To ſave the mercury on a large ſcale.

7. As ſoon as the pieces have acquired a proper degree of hardneſs to be handled without danger of breaking, which commonly takes place in a little more than an hour, place them in a proper furnace, and keep them ignited under a muffle or in a ſmall reverberatory. No other precaution is neceſſary in this operation but that of not breaking the pieces during their tranſport. The mercury flies off during the heat, and the platina remains perfectly ſolid; ſo that, after being ſtrongly ignited two or three times before the bellows, it may be forged or laminated in the ſame manner as gold or ſilver; care being taken, at the commencement of the forging or of paſſing it between rollers, not to apply too great a force till the metal has acquired all its denſity. It is almoſt ſuperfluous to add that in evaporating the mercury from large quantities of amalgam, a proper apparatus, ſuch as in the ſilver amalgamation, muſt be employed to receive the volatilized mercury; but for ſmall quantities, where the loſs of this metal is of no conſequence, the furnace muſt have a proper chimney to carry off the metallic vapours. When the platina comes out of the firſt fire its dimenſions are about two thirteenth parts ſmaller every way than the original amalgam from the mould. The whole of this operation ſeems to be governed by the preſſure of the atmoſphere and the laws of coheſive attraction: for the air is driven out from between the molecules of the platina, which by their ſolution in mercury are moſt probably in their primitive and conſequently uniform figure. It is very viſible and at the ſame time a very amuſing phenomenon to obſerve, (during the

Remarks on this proceſs.

proceſs

procefs of ignition, which is performed in four or five minutes) how the platina contracts every way into itfelf, as if preffed by fome external force.*

I have alfo lately obtained triple falts of muriate of platina with muriate of ponderous earth; and alfo with muriate of magnefia; and I ftrongly fufpect that every other earth except the filiceous, and even the metals, are fufceptible of fuch triple combinations. I have likewife obtained a very beautiful falt of platina by the combination of foda and platina with the muriatic acid; a combination which Bergman and feveral other chemifts deny. The beft manner of obtaining it is by diffolving the platina in nitrous acid, to which, for that purpofe, two parts of muriate of foda and one of platina are added. The platina muft be made in a retort with its receiver, and after about four fifths of the fluid have come over, the procefs muft be interrupted, and the whole left to cool in the fand bath. The falt cryftallizes in fine prifms, which are fometimes four or five inches long, and either red brown, like titanium, yellow, like amber, or of a beautiful coquelicot colour, according to the purity of the platina. I enclofe here my addrefs during my abfence, and hope you will receive with indulgence the contents of this letter.

Triple muriatic falts of platina, with earths and with metals;

—beautiful falt with foda.

I am with great regard, Sir,
Your moft humble and obedient fervant,
COUNT APOLLOS MOUSSIN POUSHKIN.

* In the Count's letter to Mr. Hatchett, requefting him to publifh the method in the text (communicated to Mr. H. fome years ago) the following addition is given: (in French.)

" As foon as my amalgam of mercury is made, I comprefs the fame in tubes of wood, by the preffure of an iron fcrew upon a cylinder of wood, adapted to the bore of the tube. This forces out the fuperabundant mercury from the amalgam, and renders it folid. After two or three hours I burn upon the coals or in a crucible lined with charcoal, the fheath in which the amalgam is contained, and urge the fire to a white heat; after which I take out the platina in a very folid ftate, fit to be forged."

On

II.

On Pepper. By THOMAS THOMSON, *M. D. Communicated by the Author.*

Vegetable chemiſtry neglected.

NOTWITHSTANDING the great number of labourers who have engaged in the cultivation of chemiſtry, the field of that alluring ſcience is too extenſive to be fully occupied. While many ſubdiviſions are left entirely waſte, and others exhibit here and there only faint traces of improvement, ſome fortunate ſpots, either from their ſuppoſed importance, or from the influence of faſhion or accident, have been crowded with workmen, and cultivated with enthuſiaſtic eagerneſs. The mineral kingdom has probably engaged the excluſive attention of nearly two thirds of the whole body of practical chemiſts: The animal kingdom, in conſequence of the intimate connection between chemiſtry and medicine, has enjoyed a conſiderable ſhare of cultivation; but the vegetable kingdom, though ſurely not inferior in importance, and apparently rather more alluring, has till lately been greatly neglected, at leaſt in Britain.

Reaſons why this branch muſt be always leſs accurate than mineral chemiſtry.

Indeed it is not likely that vegetable chemiſtry will ever arrive at the preciſion which we have a right to look for in the analyſis of minerals. The conſtituents of the latter ſeem ſcarcely ſuſceptible of altering their ſtate; but thoſe of vegetables run progreſſively through a regular ſuite of changes.—Thus the ſubſtance, which in the embryo ear of corn poſſeſſes the properties of *mucilage*, appears in the ripened grain under the form of *ſtarch*. Between theſe two extremes there exiſts an indefinite number of intermediate ſtates, through all of which the vegetable matter ſucceſſively runs. The rapidity and completion of theſe changes depend upon a multitude of circumſtances. In no caſe can they proceed exactly in the ſame order and at the ſame rate unleſs all the circumſtances tally. Without this the conſtituents of two vegetables even of the ſame ſpecies, cannot be in the ſame ſtate, and of courſe the analyſis of each, though conducted with the moſt perfect accuracy, will by no means exhibit an exact coincidence.

Changes in vegetable matter after life; and by reagents.

But even ſuppoſing the conſtituents of two vegetable bodies in every reſpect the ſame, ſtill the analyſis of each may lead to

to different refults. Vegetable fubftances not only pafs through a fuit of changes while they conftitute a part of the living plant, but many of them are ftill fufceptible of continuing the fuit even after they are feparated from the parent that produced them. Thus *gluten* when kept moift runs into *cheefe*; *oil* when long expofed to the fun and air hardens into *wax* or *refin*, and the milky juices of plants into *gum-refins*. Our analyfis frequently accelerates or occafions thefe changes, and even produces others altogether new. Hence the *principles* which we extract from vegetable bodies are not always the conftituents of thefe bodies; but a new fet of principles formed during the analyfis, and of courfe varying according to the nature and circumftances of the experimental inveftigation. Hence we are feldom able to form again the old vegetable compound by uniting together all the ingredients which we have extracted from it.

These difficulties increafe with the complicated nature of the vegetable body; for the greater the number of conftituents is, the more liable are they to undergo alteration during an analyfis. Indeed fome vegetable principles feem incapable of exifting except in combination, and are decompofed or new modified the inftant we attempt to feparate them. Complication of principles.

The variety of ftates in which the vegetable principles fucceffively exift, together with the difficulty of examining their properties without altering their nature, has rendered it neceffary for chemifts to apply the names of them with greater latitude than is ufual in other departments of the fcience. A general refemblance, indeed, in the moft ftriking properties, feems to have been thought fufficient to entitle vegetable principles to the fame name. If we examine all the bodies termed *gum*, we fhall find them running on the one hand into *ftarch*, and on the other into *fugar*; and forming a pretty long feries, having fugar and ftarch at the two extremities: no two of the fubftances conftituting the feries are exactly the fame; but the fame name is ufually applied to all thofe that have a general refemblance. This will be eafily feen by the following table, in which I have fubdivided the feries into five genera. Loofe nomenclature of vegetables.

Inftances in fugar, gum, ftarch.

Genus I. Sugar - -	1. COMMON SUGAR 2. Sugar of grapes 3. Sugar of beet.

II. Sarcocoll

II. Sarcocoll -	1. Manna 2. Liquorice 3. Common ſarcocoll 4. Saccharine of malt.
III. Gum - -	1. Mucilage of roots 2. Cherry-tree gum 3. Gum-Arabic 4. Gum of barks.
IV. Gluten -	1. Gluten of barley 2. Gluten of wheat 3. Caſeous principle.
V. Starch -	1. Gum tragacanth 2. Gum of lichens. 3. Common ſtarch.

Theſe terms are generic.

The terms *gum, ſtarch, ſugar,* &c. in vegetable chemiſtry are not to be underſtood as the names of peculiar ſubſtances, nor even as *ſpecies;* but merely as *genera,* and nearly ſimilar to the terms metals, acids, alkalies, &c. in the other departments of chemiſtry.

Subject introduced: *Pepper.*

The preceding obſervations will ſerve, I truſt, as an apology for the imperfection of the following remarks on *pepper.* I offer them to the public not as an analyſis of that vegetable ſubſtance, but as an account of ſome of the properties of its moſt remarkable conſtituents.

Deſcription.

I. Black pepper is the fruit of an Eaſt Indian plant, the *piper nigrum,* and is too well known to require any particular deſcription. The outer coat of the pepper-corn is brown, and a good deal ſhrivelled. Its taſte is not nearly ſo pungent as the inner part; of courſe it contains leſs of the peculiar principle to which pepper owes its taſte and ſmell.

Maceration in cold water.

II. When pepper is macerated in cold water it does not loſe its ſhrivelled appearance; a proof that the corns are impregnated with an oily ſubſtance, which prevents them from abſorbing water. The liquid very ſoon acquires a fine deep reddiſh brown colour, but retains its tranſparency. *White pepper,* which is known to want the outer coat, communicates no ſuch colour to water; the colouring matter then muſt reſide in the outer coat.

The watery infuſion thus obtained poſſeſſes the ſmell and taſte which are peculiar to pepper. Like moſt other *extracts,* it

it has the property of giving a red colour to vegetable blues. A very great quantity of water ſucceſſively applied is neceſſary to exhauſt the pepper of its colouring matter: but the ſmell and taſte of pepper become leſs and leſs ſtrong in theſe infuſions, and at laſt altogether imperceptible, leaving the infuſions inſipid, or ſlightly ſweetiſh.

1. Theſe cold infuſions contain a peculiar *extractive* matter, which ſeems to reſide in the outer coat of the pepper-corn. In the firſt infuſions their matter is united to the ſubſtance, in which the taſte and ſmell of pepper reſide, and occaſions its ſolubility in water. In the laſt infuſions, if we judge from their appearance, it ſeems to be mixed with a mucelaginous ſubſtance. Peculiar *extractive* matter.

2. If we mix the infuſion of nut-galls with the cold infuſion of pepper, no ſenſible change is produced; but in a decoction of pepper, it produces a copious flaky precipitate. Hence we learn that there is a ſubſtance in pepper inſoluble in cold water, but ſeparated by means of boiling water. This ſubſtance, as ſhall be afterwards ſhown, is a ſpecies of *ſtarch*. Cold and hot aqueous infuſions treated with infuſ. of galls.—Precip. of *ſtarch*.

3. When pepper is macerated in alcohol it communicates a light yellowiſh green colour to the liquid, which becomes at the ſame time fully impregnated with the peculiar hot principle which characterizes pepper. By diſtilling this tincture in a retort, the alcohol is obtained colourleſs, but of a decidedly peppery flavour. Towards the end of the diſtillation, the liquid in the retort becomes muddy, and depoſites a greeniſh matter, part of which may be obſerved alſo trickling down the ſides of the receiver like drops of oil. The reſidual liquid is yellow, but nearly inſipid. This green matter is the ſubſtance to which pepper is indebted for its taſte and ſmell. Its properties are analogous to thoſe of the *volatile oils*. Maceration of pepper in alcohol and diſtillation leave *volatile oil.*

4. Theſe three bodies, namely, *extractive*, *ſtarch* and *oil*, I conſider as the moſt important ingredients of the pepper-corn. Let us examine the properties of each, beginning with the oil, which is obviouſly the eſſential ingredient.

III. The colour of the oil of pepper is graſs green. When firſt obtained it is of the conſiſtence of turpentine, but it gradually hardens by expoſure to the air. When moderately heated, it gives out a liquid oil of a yellowiſh green colour, and leaves a ſolid maſs, ſimilar nearly to a reſin. When thrown into water, it ſinks to the bottom of that liquid. Characters of oil of pepper.

Its

Taste and smell strong, and that of the pepper-corn.

Its taste is intolerably hot, and precisely similar to that of pepper. So is its smell.

Habitudes. Inflammable.

When heated to 100°, it softens; it melts at 148°, evaporates a little above 212° in a white smoke, which smells like tobacco smoke, irritating the throat and exciting coughing. Evaporated to dryness on a glass plate, it leaves a yellow trace behind it. When suddenly heated, it boils violently, and the vapour burns with a clear white flame without any smoke.

Volatile by strong heat.

It gives a greasy stain of a green colour to paper. At 500° the greasy appearance is removed, but the green mark still continues, unless the heat be sufficient to char the paper. Hence I think it follows, that the colouring matter of this oil is a substance entirely distinct from the oil itself.

Soluble in alcohol and in ether, but not in water, &c.

The oil of pepper is insoluble in water; alcohol and ether dissolve it readily; the solution is light green: alcohol holding it in solution, acquires a very fragrant odour, precisely similar to that of oil of lavender. When water is added to this solution, the whole becomes milky, and passes in that state through the filter. On standing some weeks, light green flakes subside, but the milky opacity is permanent.

Action of alkalis.

Alkalies have no sensible action on this oil while cold. When thrown into liquid potash it swims on the surface. If the liquid be heated, the oil becomes brown and acquires greater consistence. At the same time it evaporates partially, diffusing around the peculiar odour of pepper.

— of nitric acid,

Nitric acid dissolves it with effervescence, the solution is yellowish brown, and a waxy matter swims on the surface. This acid acts in the same manner on other volatile oils.

— and of ox. mur. acid.

The oxygenized muriatic acid destroys the green colour and makes it yellowish white. But it still retains its former taste.

It is a vol. oil, with col. matter and resin.

These properties are sufficient to authorize us to refer this green matter to the genus of volatile oils It is not however a pure oil, for besides the colouring matter already mentioned, it obviously contains a substance which approaches to the genus of resins in its properties.

Extractive of pepper.

IV. The EXTRACTIVE of pepper is procured by macerating the pepper-corns in cold water. Like the other species of this difficult genus of vegetable principles, it is scarce possible to obtain it in a state of absolute purity. But if we macerate the pepper-corns whole in successive portions of water, till the liquid loses the peppery flavour, and then pour on them fresh

 water,

water, we obtain an infufion which I believe holds in folution fcarcely any thing but extractive. If this infufion be evaporated flowly in a fteam heat, it leaves a brown refiduum, which I confider as extractive of ftarch not far from purity.

1. The infufion is infipid or flightly fweetifh, and of a fine reddifh brown colour. As the evaporation advances, the colour deepens, and the liquid acquires an acrid flavour, fimilar to that of fcorched vegetable matter. This flavour, which indicates a commencement of decompofition, is evolved at a very moderate temperature. Aq. folution by evap. leaves ftarch.

2. If the evaporation be conducted rapidly the extractive is opaque, dark brown, and has a perceptible tafte, and delequefces or at leaft attracts moifture when firft expofed to the atmofphere. But by very flow evaporation I have obtained it in fine femi-tranfparent brown fcales, which are infipid, brittle, and not altered by expofure to the air.

3. This extractive diffolves readily in water, but not in alcohol. If it be repeatedly diffolved in water, and the folutions evaporated to drynefs, a fmall portion of it becomes infoluble in that liquid; but the greater part continues foluble after the procefs has been repeated occafionally even for months. Soluble in water, but not in alcohol.

4. This extractive is precipitated from water by moft of the metallic falts and by feveral of the earthy folutions. Precipit. by metallic falts, &c.

It is thrown down in brown flakes by lime-water ftrontian water and alum. Barytes water deepens the colour, but occafions no fenfible precipitate; nor is any precipitate produced by filicated potafs or the magnefian falts.

It is precipitated brown red by the nitro-muriate of gold, the nitrate of filver, mercury, lead and bifmuth, and by the muriate of tin and antimony. When mixed with the infufion of litmus, the colour becomes red. Ammonia reftores the original colour, and at the fame time throws down a copious blue lake.

5. When a current of oxymuriatic acid is paffed through the infufion of pepper, the brown colour is fpeedily converted into a pale yellow, and the extractive precipitates in white flakes. The eafieft method of making this experiment is the following: Put a quantity of the hyperoxymuriate of potafh into a fmall retort, having a long neck (or a flafk provided with a bent glafs tube ground into it) and pour upon it a portion —by ox. mur. acid.

tion of muriatic acid. Let the infufion of pepper be put into a tall glafs veffel, and plunge the beak of the retort to the bottom of it. A current of oxymuriatic acid is difengaged from the falt, and paffes in bubbles through the infufion for a confiderable time.

When ammonia is added to the infufion thus made white, the original red colour is reftored. The white flakes precipitated by oxymuriatic acid gas are infoluble in cold water.

Decoction of pepper affords with *tan*, a precip. foluble in hot, but not in cold water.

V. That pepper contains a fpecies of ftarch, I conclude from the following experiment, which I have frequently repeated. When the decoction of pepper is mixed with the infufion of nut-galls, a copious precipitate falls in reddifh brown flakes. If this liquid be heated to the temperature of about 120°, the precipitate is re-diffolved, but appears again when the folution cools. Now the only fubftance which poffeffes the property of forming with tan, a precipitate nearly infoluble in cold water, but very foluble in hot water, is ftarch. Tan indeed throws down gluten, but the precipitate is not re-diffolved in the application of heat. It throws down caoutchouc and fome of the gum-refins, but the precipitate is fcanty and probably owing to the extraneous matter. The precipitate which it forms with gelatine and albumen cannot even by the moft carelefs obferver, be confounded with the compound of ftarch and tan. But befides this property, which I confider as characteriftic, the ftarch of pepper, agrees with common ftarch in the phenomena which it exhibits with the different chemical reagents.

This property is characteriftic of ftarch.

Phenomena of ftarch with reagents.

As thefe phenomena have not yet been detailed by chemical writers, it may be neceffary to give a fhort fketch of them in this place.

Starch not eafily decompofed.

1. Starch is one of thofe vegetable bodies that are leaft liable to decompofition. It conftitutes one of the moft important articles of food, and acts an important part in the production of fermented liquors. The obvious properties of common ftarch are too well known to require any defcription.

Not foluble in alcohol, ether, or water.

2. Neither alcohol nor ether nor water are capable of diffolving it. The laft liquid when affifted by a boiling heat, readily unites with it, and forms a kind of jelly, which may be diffufed through boiling water; but when the mixture is allowed to ftand a fufficient time, the ftarch flowly precipitates to the bottom. By drying the compound of ftarch and water, a brittle

Forms a jelly with hot water which is little changed.

a brittle ſubſtance is obtained, differing in appearance from common ſtarch, but exhibiting nearly the ſame properties with re-agents. The apparent difference is probably owing to a portion of water remaining united to the ſtarch.

3. When ſtarch is triturated with the hot infuſion of nut-galls, a complete ſolution is effected. The ſolution is tranſparent and rather lighter coloured than the infuſion of galls. When cold it becomes opaque, and a copious curdy precipitate falls.

—Completely ſoluble in hot infuſ. of galls, and precip. by cold.

The infuſion of nut-galls, which I am accuſtomed to employ in all my experiments, except when the contrary is expreſsly mentioned, is made by boiling together one part of galls in coarſe powder and two parts of water in a glaſs retort. When cold, the liquid part is decanted into a glaſs phial. It is at firſt muddy, and opake; but on ſtanding, a ſediment falls, and a tranſparent liquid remains of a deep brown colour, which conſtitutes my infuſion. An ounce meaſure of this infuſion, when evaporated to dryneſs in a glaſs veſſel placed on a tin-plate box, heated by ſteam, leaves a brown reſidue, which weighs 68 grains. This reſidue conſiſts chiefly of tan; for the greateſt part of the extractive gradually ſeparates from the infuſion in the ſtate of a brown, tough, imperfectly ſoluble membrane. Neither *extractive* nor pure *gallic acid* has any effect upon the decoction of ſtarch. Hence the precipitate is obviouſly produced by the ſole action of tan upon the ſtarch.

The infuſion of galls as prepared was *tan.*

4. Twenty-four grains of ſtarch were triturated with half an ounce meaſure of the infuſion of galls, and mixed with about five ounces of hot water. A complete ſolution took place; but on cooling, the liquid became opaque, and a precipitate fell, which dried by a ſteam heat, weighed 35 grains. The reſidual liquor had a light yellow colour and an aſtringent taſte. When evaporated to dryneſs, it left a reſiduum that weighed 17 grains. This reſiduum contained ſtarch, for it was not completely ſoluble in alcohol. In this experiment ſome loſs muſt have been ſuſtained during the trituration. For the ſolid matter obtained weighed only 52 grains; or ſix grains leſs than the 24 grains of ſtarch and the 34 grains of ſolid matter in the infuſion of galls. The following is more to be depended on: After various trials, I found that ſtarch and tan are capable of uniting in different proportions. But the precipitate is leaſt ſoluble when $\frac{1}{4}$ oz. meaſure of infuſion of galls is uſed for every 24 grains of ſtarch.—I took 24 grains of ſtarch, boiled them

Experiment of the union of ſtarch and tan.

them in a flask with five ounces of water, and then added ¼ oz. measure of the infusion of galls. On cooling, a copious precipitate fell, and the liquid remained only faintly coloured. The precipitate dried in a steam heat weighed 31 grains, and the residual liquid left 11 grains of residue. The whole amounted to 42 grains; which is very nearly equal to the 24 grains of starch and 17 of tan employed; the compound in this case consists of

58.5	starch, or nearly	3 starch
41.5	tan - - - -	2 tan
100.0		5

The compound of starch and tan is of a light brownish yellow colour, semi-transparent and brittle, and has a good deal of resemblance to common sarcocoll. Its taste is astringent; it feels glutinous between the teeth, like gum. It is very imperfectly soluble in cold water, but hot water dissolves it abundantly. Alcohol digested on it acquires a brown colour; but is incapable of separating the whole of the tan from the starch. When heated, it froths, swells and melts, and then burns with a clear flame, leaving like starch a small portion of white ashes behind it.

Infusion of starch treated with earths.—

5. To ascertain the effect of the earths and metallic oxides on starch, an infusion of it was formed by triturating 24 grains of starch with 4½ ounces of water, and then boiling the mixture for some time. The decoction thus formed, is nearly transparent, and of a slight opal colour. When set aside, at least a month elapses before the starch begins to subside.

When lime water is mixed with this decoction, no change is produced; neither is any perceptible alteration occasioned by strontian water; but barytes water throws down a copious white flaky precipitate. This precipitate is re-dissolved by muriatic acid, but appears again on standing unless a considerable excess of acid has been added. Yet muriate of barytes occasions no change in the decoction of starch.

—with metallic salts: no effect.

No metallic salt seems to have the effect of throwing down starch from its decoction. The following were tried:

Nitro-muriate of gold, platinum;
Nitrate of silver, mercury, lead;
Muriate of tin, acetite of lead;
Salts of copper, iron, zinc;
Ammoniated nickel and cobalt.

6. When

6. When potaſs is triturated with ſtarch, and a ſmall quantity of water added, the whole aſſumes on ſtanding the appearance of a ſemi-tranſparent jelly. On adding water, an opal coloured ſolution is obtained, from which the ſtarch is readily thrown down by an acid. When muriatic acid is employed, a peculiar aromatic odour is exhaled.

When the infuſion of galls is dropt into the ſolution of ſtarch in potaſs, a yellowiſh white precipitate appears, but is immediately re-diſſolved, and the liquid remains opake and of a dark brown colour. On adding muriatic acid, a copious precipitate falls, reſembling the compound of ſtarch and tan.—Nitric acid occaſions no precipitate, neither does ammonia.

The decoction of ſtarch is neither altered by potaſh, carbonate of potaſh, nor ammonia.

Treatment of ſtarch with potaſh. Solution. Separation by acids. The infuſion of ſtarch not affected by alkalies.

7. Sulphuric acid diſſolves ſtarch, and abundance of charcoal is precipitated. Diluted ſulphuric acid, when aſſiſted by heat, diſſolves it without decompoſition. Sulphureous acid has no effect upon it.

Starch diſſolved by the ancient mineral acids. Phenomena.

Diluted nitric acid firſt reduces ſtarch to powder, and then diſſolves it, with the exception of ſome waxy matter, which ſwims on the ſurface. During the ſolution, ſome nitrous gas is exhaled.

Strong muriatic acid diſſolves ſtarch ſlowly, and without efferveſcence. When the ſtarch does not exceed $\frac{1}{20}$ of the acid, the ſolution is colourleſs and tranſparent, but if we continue to add ſtarch, a brown colour ſoon appears, and the acid loſes a portion of its liquidity. Its peculiar ſmell is deſtroyed, and replaced by the odour which diſtinguiſhes corn mills.

Acetic acid does not diſſolve ſtarch.

Experiments with alcohol.

8. Alcohol ſeparates ſtarch in part from its decoction. Potaſh diſſolved in alcohol occaſions a copious white precipitate, which is re-diſſolved on adding a ſufficient quantity of water. Alcohol digeſted on ſulphuret of potaſs, occaſions a flaky precipitate in the decoction of ſtarch: this precipitate has ſometimes an orange colour.

Starch of pepper has the preceding properties. It is united to an oil in the pepper-corn.

Such are the properties of common ſtarch: the ſtarch of pepper poſſeſſes them, excepting only that the colour of its precipitates is peculiarly modified by the other ingredients of pepper, from which it is not poſſible to free the ſtarch completely.

A very conſiderable portion of the pepper-corn ſeems to conſiſt of ſtarch, chiefly united to the oil of pepper. When a pepper-

pepper-corn is cut in two, we find it compofed of two coats. The outermoft brown fhrivelled coat is eafily feparated by fteeping the pepper in water. The fecond coat is much thinner and lighter coloured; it does not feparate by maceration. Beneath this coat is a thick zone of pale green matter, apparently ftarch united to oil. This is fucceeded by a thin yellow zone, feemingly of nearly the fame compofition. Within this is a fmall fpherical fpace, fometimes hollow, but moft commonly filled with a foft white fubftance like the pith of trees. All the zones have the peppery tafte. The outer coat has the flavour, but little of the heat of pepper.

III.

Letter from the Abbé BUÉE *on Mr.* ROMÉ DE LISLE'S *and the Abbé* HAUY'S *Theories of Cryftallography.*

(Concluded from p. 39.)

SYNTHESIS is grounded, as I mentioned, on the fact, that all well formed cryftals are terminated by plain furfaces.

Primitive forms: Since there exift primitive forms, there muft alfo be fecondary forms, for the one fuppofes the exiftence of the other. The fecondary forms are fuch, that fections can be made only parallel to the fides of the primitive; and when the primitive has been produced by thefe fections, the divifion being continued the integrant particles are obtained. (Secondary.)

Laws of the ftructure of cryftals, and theory thence refulting. The mineralogical analyfis defcends from the fecondary to the primitive form, and from the latter to the integrant particle; juft fo the mineralogical fynthefis afcends from the integrant particle to the primitive, and from thence to the fecondary forms. A cryftalline edifice is therefore raifed by means of the integrant particles. What are the laws of this extraordinary architecture? By laws I mean the difpofition of the laminæ, not the means employed by nature to execute the curious ftructure.

Laws muft exift, 1ft, for the formation of the primitive, and 2dly, for the conftruction of the fecondary form. The primitives are either fimilar to their integrant particles, or they are not. If they are not, their forms muft be parallelopi-

pedons,

pedons, and their laws of formation very simple; for there will be the same number of integrant particles in each row, as there are rows in each lamina, as there are laminæ in the primitive form. It is easy to conceive that all the joints perfectly coincide with each other, and form continued planes; neither will there be any vacuity left between the particles. If the primitive be not similar to the integrant particle, then the simplicity of the former case disappears. I have already stated that there are three forms of integrant particles; the tetraëdron, the triangular prism, and the parallelopipedon. There are also six primitive forms; the parallelopipedon, the octaëdron, the tetraëdron, the regular hexaëdral prism, the dodecaëdron bounded by rhombs all equal and similar, and the dodecaëdron with triangular sides and formed by two right pyramids united base to base. Of these six primitive forms there are only the parallelopipedon and the regular hexaëdral prism that can exactly fill up a space without leaving any vacuity. The integrant particles of the former are parallelopipedons; of the latter, triangular prisms. As to the other four primitive forms, their integrant particles are tetraëdrons. The dodecaëdron bounded by rhombs is produced by twenty-four similar tetraëdrons without any vacuity between them; the octaëdron and tetraëdron are formed by tetraëdrons leaving octaëdral vacuities; and the dodecaëdron bounded by triangles, to be formed of tetraëdrons, must imply sections parallel to more than six planes; which perfectly coincides with observation.

Laws of the structure of crystals, and theory thence resulting.

These vacuities, whose existence must be admitted in the integrant particles, as well as between those particles when forming a primitive, give rise to the following reflections:

When the elements of a substance are chemically combined, that substance is homogeneous. Let us suppose a crystal of such a substance to be subdivided into small parallelopipedons equal and similar; as the substance is homogeneous, and these little parallelopipedons having no vacuities between them, it is evident the elements that compose them are equal in number and proportion. We will next suppose the crystals of this substance can be divided by sections parallel to six planes. In that supposition, nineteen or twenty different species of parallelopipedons can be produced. Among these species some will be similar, others not; but none of the species will be exactly

Laws of the ſtructure of cryſtals, and theory thence reſulting.

actly parallel to each other. We will proceed on two ſimilar cryſtals of the ſame ſubſtance and equal in ſolidity, dividing the firſt into one ſpecies, the other into a different ſpecies, of parallelopipedons, equal in ſolidity but not in ſurface; and let the diviſion of each be puſhed to its laſt term. But as we are come by ſmooth ſections to parallelopipedons of different ſpecies, thoſe ſections have alſo produced their differences: but by ſuppoſition theſe parallelopipedons are the reſult of the laſt poſſible term of diviſion without deſtroying the chemical compoſition, and being equal in ſolidity, though not in ſurface, they cannot contain each other; therefore if their differences are not integrant parts of both, theſe differences muſt ceaſe to be homogeneous, and we come to a ſort of chemical decompoſition. It is true we cannot execute this exceſſive diviſion, but we can form a very correct idea of it. If the little parallelopipedons contain two ſorts of elements, their differences will alſo, but alſo in different proportions; and, ſir, if you will turn to Berthollet's *Reſearches on the Laws of Affinities*, you will ſee him in all his experiments proving, that however perfectly a chemical decompoſition may have been made, the reſults will always contain a certain portion of thoſe ſubſtances from which it was the object of the operation to ſeparate them. If theſe reflections, Sir, are well grounded, do they not give us hopes, and perhaps ſhow the poſſibility, of deſcending from the integrant particles to the conſtituent particles? This ſecond reſearch is of the ſame nature as the firſt. It is more than probable that the conſtituent particles themſelves are diviſible, having no determined figure, but are aggregations, ſubject to the ſame laws as the integrant particles. The object of the natural philoſopher is not to diſcover the forms of the ultimate particles, but to determine their reſpective poſitions; which, if ever they could be determined in the integrant particles and their component parts, the grand problem of chemical affinities would be fully ſolved; and ſhould ſuch ever be the caſe, to the Abbé Haüy's theory would be due the merit. The *Encyclopædia Britannica*, under the article *Chemiſtry*, in the Supplement, p. 396, ſays:

"This theory, to ſay no more of it, is, in point of ingenuity, inferior to few; and the mathematical ſkill and induſtry of its author are entitled to the greateſt applauſe.

"But

"But what we confider as the moft important part of that philofopher's labours, is the method which they point out of difcovering the figure of the integrant particles of cryftals; becaufe it may pave the way for calculating the affinities of bodies, which is certainly by far the moft important part of chemiftry. This part of the fubject, therefore, deferves to be inveftigated with the greateft care."

Laws of the ftructure of cryftals, and theory thence refulting.

But I return to the point whence this digreffion carried me, to the vacuities left between the integrant particles in the conftruction of a primitive form. The Abbé confiders them as filled either by the water of cryftallization or by fome other fubftance. It is not an admiffible fuppofition that this other fubftance is compofed of the fame elements as the integrant particles, but in different proportions? At leaft, fuch is the conclufion I fhould be tempted to draw after reading Berthollet's excellent Refearches on Affinities.

I fhall now proceed to the laws of formation in fecondary cryftals. It is eafy to deduce them from thefe two facts: *viz.* 1ft, That the fides of the fecondary cryftals are planes; 2dly, That they divide by fmooth fections parallel to the fides of their primitive form.

Let us take a rhomboid of carbonate of lime for example. If on one of the fides of the rhomboid I wifhed to raife a pyramid, I fhould lay laminæ of rhomboidal particles upon each other. Thefe laminæ would decreafe in furface until the laft is reduced to a fingle rhomboid. Thus the fecond lamina contains fewer particles than the firft, the third fewer than the fecond, and fo on. As the faces of thefe pyramids are always to be planes, the fucceffive decrements of the laminæ muft be equal; that is to fay, the fecond lamina is lefs by one range in every direction than the firft, and the third than the fecond, &c. If the decrement is more rapid; that is to fay, if two or three ranges are fubtracted in the fecond lamina, the fame number will be fubtracted from the third, and fo on fucceffively till the pyramid is completed. As the fections are to be fmooth, the joints muft form one continued plane; therefore the ranges and even the particles at the joints muft not encroach on each other: hence it follows *that the number of ranges fucceffively fubtracted from each lamina can never be incommenfurable*; that is to fay, the decrement may be 1, 2, 3, 4, &c.; but never $\sqrt{2}$, $\sqrt{3}$, &c.

Laws of the ftructure of cryftals, and theory thence refulting.

Thefe are the decrements parallel to the edges, or, as the Abbé calls them, *decrements on the edges*. But they may take place in a parallel with the diagonal of the faces of the primitive; they are then called *decrements on the angles*, becaufe the diagonals are drawn from one angle to the oppofite angle. This fecond fpecies of decrement follows the fame laws as the firft.

There is a third fpecies, called by our author *intermediate decrements*. In this cafe they are neither parallel to the edges nor to the diagonals of the faces, but to intermediate lines, which if prolonged would interfect both the edges and diagonals, but otherwife they follow the fame laws as the two firft. It is a general law, therefore, *that in all cafes the laminæ decreafe in arithmetical progreffion, and its ratio or the number of ranges fubtracted is always commenfurable.*

The particles of which the laminæ are compofed are to be confidered as parallelopipedons; not that the integrant particles always have this figure; but if they have it not, they muft leave vacuities between them, and each vacuity being added to its correfponding particle, will complete the parallelopipedon. If this was not the cafe, the faces of the fecondary cryftals would not be planes, nor could they be fplit fmoothly in any direction. Thefe little parallelopipedons which compofe the fubtracted ranges are what I called above, after our author, *fubtractive particles.*

I fuppofed the conftruction of the fecondary form only to take place on one of the faces of the rhomboid; but what was faid relative to that face is applicable to all the others. It is alfo to be remarked that different laws of decrement may affect the different faces; even further, different laws may fucceffively affect the fame face. Hence a diverfity of forms arife fcarcely credible to a perfon unacquainted with the doctrine of combinations. The Abbé Haüy has calculated, "that confining one's-felf to decrements by 1, 2, 3, or 4 ranges, and not taking intermediate or mixt decrements into account, the rhomboid is capable of 8,324,604 varieties of cryftalline forms.

It is an important remark, that whatever may be the variety of form, the forms (in complete cryftals) will always be fymmetrical. There are two forts of fymmetry, the perfect and imperfect. In the perfect, the right is fymmetrical with the left, and the top with the bottom; but in the imperfect, the top

Laws of the ſtructure of cryſtals, and theory thence reſulting.

top is not ſymmetrical with the bottom. This latter ſpecies of ſymmetry appears, by general obſervation, to be excluſively appropriated to cryſtals that become electrical by heat; that is to ſay, which being expoſed to the heat of the fire, or plunged into hot water, acquire the electric power. Theſe cryſtals, the tourmaline, for example, acquire a poſitive electricity on one ſide, while on the ſide diametrically oppoſite their electricity becomes negative; and all obſervations hitherto made give us reaſon to conclude that theſe ſides are never ſymmetrical, and are always produced by different or fewer laws of decrement. "Hence," ſays the Abbé, "by mere inſpection it is eaſy to point out which is the ſide that will give the poſitive and which the negative electricity." (Vol. i. p. 237.)

The aſtoniſhing variety in the cryſtalline forms leads us naturally to aſk, What can be the cauſe of this variety? This queſtion has not been treated by the Abbé: allow me, Sir, to ſubmit a few ideas on the ſubject for the opinion of mathematicians.

Firſt cauſes, I repeat, are not the object of this diſcuſſion. I ſtate the queſtion thus: Why does the ſame ſubject cryſtallize in ſuch a variety of forms, always ſymmetrical and always terminated by planes?

The ſolution of this queſtion ſeems to require three conditions: 1ſt, That the particles of the ſubſtance diſſolved in the fluid all leave the ſtate of reſt at the ſame inſtant, to form the cryſtal by their aggregation: 2dly, That, while theſe particles are in the act of drawing near to each other, no foreign power ſhall imprint on them any other motion than a common motion, whether it be in a ſtraight line, or rotary round their common centre of gravity: 3dly, That the particles all arrive at the ſtate of reſt at the ſame inſtant, which takes place when the act of cryſtallization is finiſhed. The ſecond condition is neceſſary, and infers the firſt and third. The natural conſequence of theſe conditions will be, that the aggregation of the particles will only take place conformably to a law acting equally on all of them, whatever may be the law.

Since they all leave the ſtate of reſt at the ſame inſtant, they are in equilibrio previous to that inſtant. Since they all arrive at the ſtate of reſt at the ſame inſtant, they are in equilibrio after that inſtant: but when particles that are acted upon by

Laws of the ſtructure of cryſtals, and theory thence reſulting.

no other force than that which they exerciſe on each other, are in equilibrio, they are in the cloſeſt poſſible union that concomitant circumſtances will permit. If the particles were in equilibrio previous to their leaving the ſtate of reſt, ſomething muſt have obſtructed their approach. Let us ſuppoſe that *ſomething* to be the interpoſition of another ſubſtance, and that ſo long as the interpoſition remains equilibrium is maintained. But this can only be the caſe, in as much as the whole of the particles of the interpoſed ſubſtance are in equilibrio with the whole of the particles diſſolved and about to leave the ſtate of reſt, which in the future I ſhall call the *proper particles.* If by any cauſe which acts uniformly on the whole ſurface of the diſſolving fluid any of the interpoſed particles are ſubtracted, the proper particles muſt ceaſe to be in equilibrio. A ſtep toward aggregation will immediately take place, and the equilibrium will be reſtored. A further ſubtraction will produce a further ſtep toward aggregation, and a conſequent equilibrium; and theſe operations will be repeated ſo long as the cauſe of ſubtraction continues, and the longer its duration the larger will be the reſulting cryſtalline maſs. If the above mode of reaſoning be admitted, it will ſuffice to apply the laws of equilibrium to deduce the laws of cryſtalline forms. The laws of equilibrium to which I allude, are thoſe of the equilibrium of fluids, with certain modifications which ſhall hereafter be explained. According to theſe laws, that the preceding conditions may take place in the formation of a cryſtal, it will be neceſſary that they take place in the formation of each and every part of it, whatever may be the figure or the ſmallneſs of thoſe parts. They muſt alſo take place in thoſe laſt cryſtals which contain the leaſt poſſible number of particles; and as theſe particles are in equilibrio, and in the greateſt poſſible ſtate of proximity to each other which circumſtances will permit, it muſt follow, to fulfil all the conditions, that theſe particles form a ſymmetrical polyëdron. This peculiar diſpoſition of the cryſtalline particles conſtitutes the modification, to which I alluded, in the laws of the equilibrium of fluids; it being neceſſary in this caſe to take the number of cryſtalline particles into account, which is not the caſe when treating of the particles of a fluid. In a fluid, the particles and their reciprocal diſtances are ſuppoſed infinitely ſmall; but the cryſtalline particles and their diſtances to each other

other must be supposed finite. This material difference will necessarily cause a difference between the forms of their aggregates. Those formed with the particles of a fluid will be bounded by curved lines; the crystalline aggregates, on the contrary, will be terminated by straight lines; and when these straight lines are not too small, the boundaries will be sensibly rectilinear.

Laws of the structure of crystals, and theory thence resulting.

To ascertain what the power is that holds the particles in the state of rest, though not in close contact, is not the question; but the form of the polyëdrons which they produce. The closer adhesion of the particles to be obtained by the subtraction of caloric, sufficiently demonstrates that the particles are not in close contact with each other, and the constancy of the crystalline forms equally proves that they are in equilibrio. We will now proceed to the construction of a crystal with these crystalline particles. That the constancy of the form in the large crystal be preserved, the particles must be in equilibrio. That the equilibrium be preserved, the forces that solicit the particles to motion must mutually destroy each other. That the mutual destruction of those forces be effected, these forces after having been decomposed into other relatively parallel to three axes perpendicular to each other, and having a common point of intersection, must each meet in its direction another force equal and diametrically opposed to it. This will be obtained if the similar particles are arranged on straight lines parallel two and two at equal opposite distances from the common centre, and bisected by lines passing through that centre; but if the particles are thus arranged, they must produce symmetrical solids bounded by planes; and they are thus arranged: for if a foreign force, an excess of caloric for example, does not impede the free arrangement of the particles in the formation of the crystal, their exterior disposition will follow as much as possible their interior arrangement; but their interior arrangement must be on straight lines, or the crystal would cease to be homogeneous; their exterior disposition will therefore be on straight lines.

As the circumstances giving rise to the approach of the particles may be in the highest degree variable, it must follow that the forms produced may be diversified in the extreme. Such, Sir, is the answer I should submit for the solution of the question proposed.

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Laws of the ſtructure of cryſtals, and theory thence reſulting.

When ſpeaking of the approach of the proper particles, I ſaid that it might be occaſioned by the ſubtraction of certain interpoſed particles which obſtructed the approach of the proper particles. The former are generally water, caloric, or any fluid elaſtic or not. Their exit may perhaps make place for others, ſuch as light, electricity, &c. &c. But the eſſential point is, that whatever theſe particles may be, they are in perfect equilibrio with the proper particles, otherwiſe they would become perturbing forces. Hence it follows, that not only the integrant particles of the cryſtal, but all thoſe that are mixed with them, the chemical or component particles and even the vacuities, muſt follow the ſame laws. It alſo follows, that if each ſpecies of particle (even the chemical) that enters into the formation of the cryſtal be ſeparately conſidered, each ſpecies will have its diſtinct ſymmetrical and polyëdral form. The forms will penetrate each other, while the particles will not only not penetrate, but not even touch each other. All forms would ſtand in the ſame predicament as the regular octaëdron, which contains, as the Abbé Haüy has demonſtrated, ſix regular octaëdrons and eight regular tetraëdrons, each tetraëdron containing one octaëdron and four tetraëdrons. It will further follow, if the chemical elements can be looked upon as particles which are not in contact with each other, that we may from thence mathematically determine chemical affinities.

I have now, Sir, but one taſk left; to ſpeak of the application our author has made of algebra and geometry to cryſtallography. Many perſons complain of the difficulty neceſſarily reſulting from it in the ſtudy of mineralogy; and dare not engage in it, uncertain whether they will find a compenſation for their trouble. Our author has therefore adopted a double plan, and begins by expoſing his theory by a ſeries of reaſonings and arguments which will ſuffice to make the reader underſtand it, or any diſcoveries made in conſequence of it. He then expoſes the theory in the moſt correct of all languages —mathematical analyſis; by far the moſt intereſting, and the only means of making diſcoveries one's-ſelf: and who can be callous to the pleaſure of diſcovering an unknown truth? If the ſolution of a problem gives ſo much ſatisfaction, though the data be only imaginary, what muſt be the ſenſations of thoſe who are happy enough to ſolve problems whoſe data are

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set by *Him* whom the greatest of pagan philosophers calls the *Eternal Geometrician?* This recalls reflections to my mind which I cannot suppress. Conversing one day with the Abbé Haüy, he was taking a cursory view of all the modern discoveries; when he could not help remarking, that there was not one of them but what furnished victorious arms to the cause of religion. My answer was, that in future the name of God would be as distinctly written on a crystal as it had hitherto been in the heavens. The observation of this most religious and ingenious man reminds me of the saying of lord Bacon: "A little philosophy estranges us from religion, but a great deal reclaims us again." Even d'Alembert could not help saying, "An atheist in the Cartesian system is a philosopher mistaken in the principles; but an atheist in the Newtonian system is something worse, an inconsequent philosopher."

Laws of the structure of crystals, and theory thence resulting.

But to return to the mathematical part of our author's theory: the branch of mathematics, and the manner in which he treats it, are almost new. The theory of polyëdrons had been nearly neglected by geometers, both on account of the difficulty to represent a polyëdron on a plane, and because they did not feel the utility of the pursuit. Nevertheless, strange to say, all the regular figures that are to be found in one of the three kingdoms of nature are polyëdrons. In this point of view, the branch of mathematics illustrated by the Abbé becomes very interesting; and it is not a little so, to see with what ingenuity he extricates himself from the difficulties he meets with in his researches. He forms all the polyëdrons, however complicated, of little equal rhomboids or parallelopipedons, and by that means he reduces the theories of every possible polyëdron to that of the rhomboid, which is extremely simplified by two very simple remarks: 1st, That in all equilateral rhomboids, whatever may be the species, their projection on a plane perpendicular to their axes will always be a regular hexagon: 2dly, That the axes will always be trisected by perpendiculars drawn from all the lateral solid angles. His theory has also led him to discover in a variety of crystals geometrical properties, which must be highly gratifying to geometers. But the great advantage to be derived from it is, that it enables us with the fewest possible data to calculate the crystalline forms just as astronomers do the motions of the

heavens.

Laws of the ſtructure of cryſtals, and theory thence reſulting.

heavens. By the very means by which the latter determine the future motions of the heavens, the Abbé decides which forms are poſſible and which are impoſſible. It is thus by his ſimple and general law of cryſtallization, "the number of the ranges of the ſubtractive particles muſt always be a commenſurable quantity," that he has demonſtrated the regular dodecaëdron and the regular icoſaëdron to be impoſſible forms in mineralogy. As the immortal Newton, by having diſcovered the law of attraction to be "in the inverſe ratio of the ſquares of the diſtances," explained and calculated every thing in the vaſt regions of the firmament; ſo at the other extremity of the creation the Abbé Haüy, by means of a ſingle law which he has diſcovered, explains the irregularities and calculates thoſe problematic formations with which the mineral kingdom had hitherto aſtoniſhed the natural philoſopher.

Laws, Sir, that reſult from the ſtudy of nature, enjoy this ineſtimable advantage, that they always lead to *equations*; and it is only by the help of equations (expreſſed or underſtood) that queſtions can be ſolved which relate to objects that can be either counted or meaſured.

Of late, Sir, the word *nature* has been ſo much abuſed, that I muſt beg leave to ſtate the preciſe ſenſe in which I wiſh to be underſtood whenever I made uſe of that word in the courſe of this letter. The Abbé Haüy found it neceſſary to take a ſimilar precaution at the beginning of the excellent work *(Traité de Phyſique)* he has lately publiſhed. He ſays: "This word NATURE, ſo frequently in our mouths, can only be looked upon as an abridged expreſſion, either for the reſult of thoſe laws which the GREAT CREATOR has imprinted on the univerſe, or for that aggregate of beings the works of his hands. Nature, thus viewed in its true light, is no longer a ſubject of cold and ſterile ſpeculation. The ſtudy of its productions, of its phænomena, ceaſes to be a mere exerciſe of the mind; it moves the heart, and ſtrengthens the moral virtues in man, by awakening in his mind ſentiments of reſpect and admiration at the ſight of ſo many wonders bearing the viſible characters of infinite power and wiſdom."

With theſe ſentiments I remain, Sir, your's,

A. Q. BUEE.

July 13, 1804.

Obſervations

IV.

Obfervations on Mr. GOUGH'S *Strictures on the Doctrine of Mixed Gafes, &c. In a Letter from Mr.* J. DALTON.

To Mr. NICHOLSON.

SIR,

FROM the formidable manner in which Mr. Gough opens the campaign, I might expect him to bring a hoft of *facts* and *arguments* to his affiftance; the *facts*, however, he prudently keeps in referve, and it is to be feared feveral of the *arguments* too, as thofe already drawn out are fcarcely of force fufficient to provoke refiftance. Introduction.

Mr. Gough's firft *argument* is, that the fubject in difpute is more properly denominated *hypothefis* than *theory*. This is certainly not worth contending about.

Mr. Gough's next *argument* is, that the hypothefis is not mechanical; but as this cannot be made good, it feems, without a geometrical difquifition of length much exceeding an ordinary letter, the philofophical world are to wait till the fame fhall appear in due courfe. I might therefore, for the prefent, wave any remarks on this head, as a long and equally abftrufe defence might be expected. But as I think geometry has nothing to do in the bufinefs, and that all that can be faid effectually, either for or againft the hypothefis, being confiftent with mechanical principles, may be comprized in one fhort paragraph, I fhall difcufs the argument here. Whether the hypothefis be mechanical.

Oxigen repels oxigen, but not azote: This is a poftulatum; and being admitted, it follows, that if a meafure of oxigen be put to one of azote, the oxigen finding it porous, muft enter the pores, and *vice verfa*, till the two gafes feverally making their way into the interftices of the other, at laft obtain a perfect equilibrium, and then prefs with equal force on all the furrounding bodies, and no longer prefs on each other. This is fo plain and obvious an inference, and fo little involves any mechanical confideration, that I fhould have juftly incurred blame for infulting my readers with the appearance of mathematical demonftration in the cafe. As well might I have attempted, from the elements of Euclid, Poftulate. The parts of a gas repel each other, but not thofe of other gafes.

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to demonſtrate to a cottager, that if he put a ſieve over his chimney the ſmoke would ſtill eſcape, though interruptedly; or to a chemiſt, that if he drilled holes in an exhauſted receiver, it would in time be completely filled with air.

On the action of aqueous vapour on water.

Mr. Gough next remarks upon my opinion, that the atmoſphere of aqueous vapour is ſufficient to prevent the ocean from eſcaping into the air, which he ſeems to think wonderful: Upon this I may remind him of another wonderful fact, which I conſider the ſame in effect; that is, the preſſure of like vapour on a cup of water in an exhauſted receiver, prevents the water from eſcaping out of the cup. When Mr. Gough ſhall explain how this fact is to be accounted for, I may avail myſelf of his explanation to apply to the other. He proceeds, however, to demonſtrate the impoſſibility of an aqueous atmoſphere; but inſtead of that, he demonſtrates *the impoſſibility of an atmoſphere of any kind preſſing on water, without at the ſame time forcing the water up into its pores.* Luckily, as Mr. Gough obſerves, the facts do not countenance the concluſion; and therefore, however rigid the demonſtration, it ſhews that the previous data are not correctly aſſumed. The conſideration of this ſubject is, notwithſtanding, an important one; it is more than a year ſince I urged Mr. Gough to pay attention to it, and to attempt a ſolution of the difficulty which perplexes all theories of the atmoſphere alike: I did it the rather, becauſe I thought him well qualified for a ſubject of this nature, where the aid of mathematical ſcience muſt be of ſubſervience. The ſubject has never been at all explained that I know of. I have made an attempt which has not yet been publiſhed, except in a lecture laſt winter; but I ſhall ſtill be glad to avail myſelf of any aſſiſtance I can obtain.

More particular ſtatement of the fact; and queſtion.

Suppoſing that in the loweſt ſtratum of an atmoſphere incumbent on water, there is *one* particle of gas for *one hundred* of water, (which may be the caſe, conſidering their relative denſities as *one* to a *thouſand)*; *query,* How does the air diffuſe its preſſure over all the hundred particles equally, in ſuch ſort that no column of particles of water is forced up into the interſtices of the atmoſphere by the inequality of the preſſure? If Mr. Gough will explain this, either for his new theory or for any other, I will engage to remove all difficulty on this head which attaches to my hypotheſis.

Mr.

Mr. Gough next wants a rigorous proof of the propofition, that one gas affords an impediment to the motion of another, but that two fuch fluids finally overcome their mutual obftructions, and occupy the fame fpace in a ftate of perfect independence. No one acquainted with the experimental part of pneumatic chemiftry would have required proofs of fuch facts, becaufe he daily experiences them. Take two phials filled with different gafes; apply the mouth of one to that of the other for a few moments; upon withdrawing it, the two phials will be found to have interchanged very little of their contents. This is a moft unqueftionable proof that gafes afford impediment to each other's motion; for, into a perfect vacuum the air rufhes inftantaneoufly. Again, let the phials remain in connection for a few minutes, or at moft, hours, and they will be found to have both gafes in the fame proportion; and this ftate will continue in perpetuity afterwards. That they are ultimately *independent* on each other, is fufficiently marked by the circumftance, that any fubftance having an affinity for one of them will withdraw it from the mixture, if it will take it alone.

Impediments afforded by one gas to the diffufion of another.

Mr. Gough proceeds to give a *new theory of mixed gafes*: At the commencement of this controverfy he came forth to *defend* the all-fufficiency of the old doctrine of the chemical union of water and air, and the homogenity of the atmofphere, and to *attack* the new doctrine, which profcribes chemical union in thefe inftances, and places for its fundamental and diftinguifhing maxim, that "mixed gafes neither attract nor repel one another:" It was therefore with no fmall furprife that I found, upon his advances, that a *new theory* was ftill requifite; but this furprife became aftonifhment, when I found the preamble admitted the exiftence of certain, "gafes that neither attract nor repel each other." The only clear information I could obtain from this fudden revolution was, that Mr. Gough ufes the terms *theory* and *hypothefis* in a contrary fenfe to what philofophers in general do. When I brought my *hypothefis* (as he calls it) forward, it was fupported by an extenfive train of facts, the refult of long and careful inveftigation, none of which, that I know of, has fince been controverted; I mean what Mr. Gough alludes to as a "number of probabilities of an experimental nature:" Whereas Mr. Gough's *theory* is propofed without a fingle

Mr. Gough's new theory of mixed gafes.

a single fact to corroborate it, merely to try how far it may be found to agree with facts already known. Leaving Mr. Gough to develope his theory, which I confess I do not comprehend, and therefore cannot follow him in the application, I proceed to his concluding remarks on the law of affinity, which connects water with the different gases.

Water suspended or diffused alike in all gases.

Saussure, to whom Mr. Gough refers, in the ninth chapter of his second essay instructs us, that the *solvent powers* of common air, carbonic acid, and hydrogen, are the same as far as his experience goes. The results of Kirwan's experiments I am unacquainted with. Clement and Desorme, in the forty-second volume of the *Annales de Chimie* for 1802, have clearly shewn, that all gases take up the same quantities of water, alcohol, and ether, in like circumstances; and I think I have shewn, that these quantities are precisely the same as a torricelliam vacuum of the same capacity takes up: So that unless Mr. Gough can hold out some further encouragement to the resumption of the enquiry, it seems hardly likely that any one will undertake to investigate the *diversities* in the law of affinity between gases and water, when there does not appear a single fact that points out any affinity at all in the case.

I am your's, &c.

J. DALTON.

Manchester, Sept. 8, 1804.

V.

Account of the Striking Part of an Eight-Day Clock. By Mr. John Prior, *of Nessfield, Yorkshire. Communicated to the Society of Arts.* *

To CHARLES TAYLOR.

SIR,

New striking part of a clock.

I HAVE taken the liberty of sending to you a new striking-part for an eight-day clock, of my own invention and workmanship, which I finished last April; * * * * * *. I beg leave

* Extracted from their Transactions, 1803. A reward of thirty guineas was given by the Society, who have a model of the same.

leave to inform the Society of some of the advantages arising from this new invention. New striking part of a clock.

First, it consists in a wheel and fly, with six turns of a spiral line, cut upon the wheel, for the purpose of counting the hours. The pins below this spiral elevate the hammer, and those above are for the use of the detent.

This single wheel serves the purpose of count wheel, pin wheel, detent wheel, and the fly wheel, and has six revolutions in striking the twelve hours.

Permit me to suppose a train of wheels and pinions, used in other striking parts, to be made without error, and that the wheels and pinions would turn each other without shake or play. Allowing the above supposition to be true, which every mechanic knows it is not, my striking part will be found six times superior to others, in striking the hours 1, 2, 5, 7, 10, 11, and twelve times, in striking 4, 6, 8, and eighteen times, in striking 3, 9, and 12.

I have designedly made a defect in the model herewith sent, in striking 2 and 3 o'clock, to shew that what I have now advanced will, upon the trial, be found to be true.

In striking 2, I have purposely made an imperfection, equal to the space of three teeth of the wheel; and, in striking 3, an imperfection of nine or ten teeth; and yet both these hours are struck perfectly correct.

The flys in clocks turn round, at a mean, about sixty times for every knock of the hammer, but mine turns round only three times for the same purpose; and suppose the pivots were of equal diameters, the influence of oil on them would be as the number of revolutions in each.*

It would be better for clocks if they gave no warning at all, but the snail-piece to raise a weight somewhat similar to the model now sent for the inspection of your respectable Society.

Reference to Mr. PRIOR's *Striking Part of his Clock.*
Plate VI. Fig. 1.

A. The large wheel, on the face of which are sunk or cut the six turns of a spiral.

B. The single worm screw, which acts on the above wheel, and moves the fly C.

* See Cummings's Elements.

D.

New ftriking part of a clock.

D. The fpiral work of the wheel A. The black fpots fhow the grooves into which the detents drop, on ftriking the hour.

E. The groove into which the locking piece F drops, when it ftrikes one, and from which place it proceeds to the outward parts of the fpiral in the progreffive hours, being thrown out by a lifting-piece H, at each hour; the upper detent G being pumped off with the locking-piece F, from the pins in the wheel A.

In ftriking the hour of *twelve,* the locking-piece, having arrived at the outer fpiral at H, rifes up an inclined plane, and drops by its own weight to the inner circle, in which the hour *one* is to be ftruck, and proceeds on in a progreffive motion through the different hours till it comes again to *twelve*.

I. The hammer-work made in the common way, which is worked by thirteen pins, on the face of the fpiral.

Fig. 2.—K. The thirteen pins on the face of the fpiral, which work the hammer-work.

L. The outer pins, which lock the detent.

M. The pump-fpring to the detent.

VI.

On the Coft of making Phofphorus. In a Letter from J. P.

To Mr. NICHOLSON.

SIR,

Briftol, September 16, 1804.

HAVING lately been led to confider the various proceffes by which phofphorus might be obtained, with a view to determine the moft eligible, I thought that a very fhort fketch of what had occurred to me on the fubject, might be not improperly introduced in a fpare corner of your Journal; if you fhould be of that opinion, the contents of the following pages are much at your fervice, from a very refpectful admirer of your unremitting exertions in the caufe of Science and Philofophy.

J. P.

1. In

1. In the ufual method of procuring phofphorus from bone-duft, and fulphuric acid, the value of the ingredients is very fmall, the labour of the procefs, with the fuel employed for the evaporation and diftillation form the principal confideration in eftimating the value of the product. Eftimate of expence of obtaining phofphorus from bones. Refult 3s. per oz.

One pound of bones calcined and pulverized with half a pound of fulphuric acid, will produce, according to Fourcroy, about three quarters of an ounce of phofphorus; if the labour and fuel be eftimated at two fhillings, and it cannot be probably much lefs, the phofphorus would then coft the manufacturer about three fhillings an ounce.

2. If to the acid folution obtained by the above method be added $1\frac{1}{4}$lb. of acetite of lead, value three fhillings, the phofphate of lime will be decompofed, and two ounces of phofphorus fhould be produced from the precipitate, (*vide* Fourcroy.) This appears then to be an improvement, fince $1\frac{1}{4}$oz. of phofphorus is procured for three fhillings. But perhaps the following procefs may be preferable to either. The additional phofphorus obtained by adding acetite of lead, is rather cheaper.

3. To one pound of phofphate of foda, value two fhillings and fix-pence, add $1\frac{1}{4}$lb. of acetite of lead, value three fhillings; above one pound of phofphate of lead will be immediately precipitated, and about one pound of acetite of foda may be eafily obtained by evaporation. If this fhould be valued at two fhillings and fix-pence, the original coft of the ingredients will be reduced to three fhillings, add one fhilling for fuel and labour, and two ounces of phofphorus will be procured for four fhillings. It is needlefs to remark the great fuperiority of the laft method in point of facility, neatnefs and fimplicicy. The procefs of decompofing phofph. of foda by acet. of lead affords phofphorus at 2s. per oz.

VII.

On the Purification of Water by Filtration; with the Defcription of a fimple and cheap Apparatus. In a Letter from SIR HENRY C. ENGLEFIELD, *Bart. F. R. S. &c.*

To Mr. NICHOLSON.

SIR,

I CANNOT but think the filtering machine of Profeffor Parrott, publifhed in your laft Journal, a very inconvenient form of a very common inftrument; but as the filtration of the water Obf. on P. Parrott's filtering machine, &c.

water with which the metropolis is ſupplied contributes materially to the health and comfort of thoſe who uſe it, and in fact renders it purer than almoſt any known ſpring water, a very cheap and commodious apparatus for the purpoſe may be conſidered as an object of general utility. I therefore ſend you a ſection of a machine which I conſtructed ſeveral years ſince, and which any common carpenter can make for a very few ſhillings, which may be thoroughly cleanſed at any time, and which occupies very little room.

As you have already honoured ſeveral of my ſhort eſſays, publiſhed in other collections, with inſertion in your Journal, you will, I truſt, not be diſpleaſed that we ſhould become immediate correſpondents.

I am, Sir,

Your obedient Servant,

H. C. ENGLEFIELD.

P. S. I will not anſwer for it that the machine I ſend you has not been already introduced into uſe. If it has, you will of courſe ſuppreſs my letter; but if it has, Profeſſor Parrot's filtre is quite nugatory.

Arrangement for purifying water on a large ſcale.

A moſt excellent arrangement for the purification of river water on a large ſcale is mentioned in the writings of De Luc or De Sauſſure, but I cannot turn to the paſſage in their works. It was applied with complete ſucceſs by the inventor, to the ſtream which ſupplied a large town in Switzerland. The machine (if it may be ſo called) was as follows:

A is the upper ſurface of the ſtream to be purified, B the bottom. A ciſtern is ſunk of ſix or ſeven feet in depth, and of a proper breadth, divided by parallel partitions, alternately riſing above the ſurface level of the ſtream and open at the bottom, and level with the bed of the river, and cloſed at the bottom. It is obvious that the courſe of the water muſt be in the direction of the arrows, and in this repeated and ſlow aſcent and deſcent, all floating impurities will be left at the top, and the heavier mixtures will ſubſide. The ciſtern may be eaſily cleanſed, either by taking out the partitions, if it is on a ſmall ſcale, or by ſending perſons down between the walls, if it is built permanently for a great ſtream. Perhaps, indeed, a box having the partitions filled half the way up with ſand or gravel, may on this plan be the beſt of all filtres for domeſtic uſe.

Deſcription

Description of the Filtering Apparatus, Plate V. *Fig.* 1.

A B The exterior tube, two feet high and fix inches fquare within. Filtering apparatus defcribed.

C D The interior tube, four inches fquare at the top and three inches at the bottom. It reaches within about three inches of the bottom of the exterior tube, and is covered at the bottom with a coarfe linen tied round it. The ufe of this is to prevent the weight of the water from difturbing the fand. The upper end of this tube is formed into a funnel, for the convenience of filling it with water, and it refts on the outer tube. E a fpout for the exit of the filtered water.

Both tubes are filled with clean wafhed fand up to the dotted line juft below the fpout. A bag for ftopping the coarfer impurities may be adapted to the funnel. If this machine be placed under the cock of any common water ciftern, which is opened juft enough to fupply the funnel without running over, it will require no attendance, and will very feldom want cleaning. It is obvious that every part of the machine, when the two tubes are taken afunder is vifible to the eye, and eafily reached by the hand. The fand, when wafhed, will ferve many times.

If inftead of a funnel, a larger refervoir of water at the top is ufed, which may fometimes be convenient, it will be beft to fill the upper part of the inner tube for a few inches with clean fmall pebbles, as the pouring in water difturbs the upper furface of the fand.

It may be made either of wood or tin, but not of lead, for fear of impregnation. It is alfo evident that the rapidity of action of the filtre will be in a great degree regulated by the difference of level between the fpout and the furface of the water in the funnel, and by fupplying the funnel with a greater or leffer ftream, the machine may be made to act as quick or as flow as is wifhed.

As the water which fupplies the metropolis is often tainted with vegetable or animal fubftances putrified in it, it might be well worth while to try whether filling the inner tube with powdered charcoal might not tend to free the water filtred through it from the difagreeable tafte and fmell communicated by the caufes above-mentioned. It will alfo be advantageous to place the receiving veffel at fome diftance below the fpout, that the ftream may fall through as much air as it conveniently can. Whether charcoal would be advantageous in a filtering apparatus for water.

VIII.

Experiments on the Effects of Heat modified by Compression, by SIR JAMES HALL, *Bart. Read in the Royal Society of Edinburgh, August* 30, 1804. *Communicated by the Author.*

Experiments of heat, modified by compression.

I BEG leave to announce to this Society the result of a series of experiments which have occupied my attention almost exclusively during several years. These experiments relate to the effects of heat modified by compression, and are intended to investigate the peculiar and characteristic principle of the Huttonian theory.

Dr. Hutton's theory of mineral compounds naturally produced in this way.

Dr. Hutton, in common with many former geologists, has ascribed the formation of all mineral substances chiefly to fire. But, according to him, the influence of this element has been very much modified by compression, occasioned by the weight and strength of a vast superincumbent mass, which then pressed upon what is now the surface of our globe. In this manner he has anticipated the natural objection to all igneous theories which must arise from a comparison of various mineral substances with the products of fire in our furnaces; for he conceives that pressure by repressing volatility would occasion the presence, in high temperatures, of many substances which escape in our fires, on a slight application of heat; and that these by their chemical relations would give rise to a state of things untried in any experiments hitherto published, but such as to afford a satisfactory explanation of all the natural phenomena upon his hypothesis, even of those which are the most incompatible with the common action of fire.

The facts are that heat has so acted in our globe; but the effects have not been shewn by experiment.

The two fundamental *postulata* required in this theory, namely, the action of heat, and the presence of a superincumbent mass are certainly allowable; since the volcanos furnish us with a proof that internal fire does act occasionally and in the irregular manner which this system requires; and since the fragmented and perturbed state of our strata enables us to say with certainty that great changes have taken place, that enormous masses have been removed, and that what was once placed at a great depth is now highly elevated.

But a third *postulatum* is involved in this theory, which seems to be of more difficult admission. Granting that heat did act on

on ſubſtances conſtrained by preſſure, would its action be modified? Would that modification be ſuch as is aſſumed in the Huttonian theory?

Dr. H. has endeavoured to eſtabliſh theſe effects from geological obſervations.

To theſe queſtions Dr. Hutton has replied by arguments founded on general analogy; and has reſted the proof of his hypotheſis on its agreement with the phenomena of nature. In that reſpect few philoſophical theories have been ſo fortunate; for its univerſal application to every department of the mineral kingdom, and its ſolution of all the difficulties, afford a concurrence of probabilities in its favour which preſs on the mind with almoſt irreſiſtible conviction. Still it muſt be owned that the baſis of the ſyſtem is hypothetical; and a wiſh has been expreſſed by every man of ſcience who has attended to the ſubject, that this baſis ſhould be ſubmitted to the teſt of experiment.

The author recurs to actual experiment.

My object has been to accompliſh that end, and to bring this great queſtion to an *experimentum crucis*. By placing ſubſtances in the predicament aſſigned to them in the Huttonian theory, I have endeavoured to imitate the ſuppoſed proceſs of nature. In this attempt I have met with great and numerous difficulties, but I have at laſt ſucceeded beyond my original expectation, and have obtained reſults, which, if I am not greatly deceived, eſtabliſh as a law of chemiſtry the moſt paradoxical of Dr. Hutton's poſitions.

Carbonate of lime agglutinates (at 22° Wedgwood) in ſtrong cloſe veſſels, and becomes limeſtone.

My experiments ſhew, that when pounded carbonate of lime, produced by the trituration of chalk, of marble, of the ſhell of a fiſh, or of calcareous ſpar, after being rammed into a ſmall tube of porcelain, is expoſed in veſſels of ſufficient ſtrength and tightneſs to the heat of 21 or 22 of Wedgwood's pyrometer (that is to the heat in which pure ſilver melts,*) the

* I take this opportunity of mentioning that a very material error ſeems to prevail with reſpect to this point of the pyrometrical ſcale. The error is the more formidable that it has been introduced and ſanctioned by the higheſt authority poſſible in ſuch a caſe; I mean that of Mr. Wedgwood himſelf. In his account of the pyrometer he gives a table, expreſſing the effects produced at various points of temperature, and ſtates 28 as the melting heat of ſilver. Now it conſiſts with my knowledge that pure ſilver melts at 22. I learned the fact from Dr. Kennedy, and I have had occaſion to confirm the truth of it in numberleſs trials.

the carbonate ſhrinks upon itſelf and agglutinates into a firm maſs, which in point of hardneſs and ſpecific gravity approaches very near to common lime-ſtone, and ſometimes equals it, and which has frequently acquired the ſparkling fracture, the ſemi-tranſparency, the ſuſceptibility of poliſh, and the general aſpect of marble. The ſame reſult is obtained when a ſolid piece of chalk is treated in a ſimilar manner, and the chalk being previouſly meaſured in Wedgwood's gage, is found to contract during the action of heat three times more than the pyrometer pieces do in the ſame temperature. During the action of heat, the carbonate is found to have loſt very little of its weight; that loſs amounting in many caſes to leſs than one per cent. and in ſome experiments it has undergone no ſenſible loſs at all, or ſo very ſmall a one, that it may be neglected without fear of error. When thrown into an acid, this artificial limeſtone effervesces violently as it diſſolves, the diſcharge of gas continuing whilſt the ſmalleſt atom of carbonate remains viſible.

Great contraction of the chalk.

Imperfect fuſion of the chalk.

I have been in poſſeſſion of this fact ſince the year 1801, and I long attempted in vain to carry the experiment farther, ſo as to accompliſh the fuſion of the carbonate. In one ſolitary and accidental inſtance, I had ſucceeded in obtaining it in a ſtate of real froth, which could not have been produced without previous liquefaction; but being unable to repeat this reſult, I was unwilling to publiſh it or any of the facts already ſtated, till I could do ſo in a more ſatisfactory manner. In the courſe of laſt winter, with the help of many improvements in my mode of operation, and of ſtronger apparatus, I at laſt acquired the power of performing repeatedly and even with tolerable certainty, what at firſt had been the effect of chance.

Actual fuſion.

In theſe experiments carbonate of lime has not only been agglutinated,

This obſervation relates to the pyrometer pieces ſold by the late Mr. Wedgwood, which were formed of a mixture of alumine with Corniſh porcelain clay. This ſet having been the only one ever diſtributed amongſt chemiſts, muſt certainly be looked upon as the ſtandard. Other ſets had previouſly been made by him of Corniſh clay alone, which had never been ſent abroad, or at leaſt only given to ſome friends. It is poſſible that the diſcordance alluded to in this note, may have been occaſioned by experiments made with thoſe firſt ſets, which may have poſſeſſed different properties from thoſe afterwards ſold.

glutinated, but actually fused; the fubftance finking upon itfelf with a round and gloffy furface, and exhibiting every proof of a vifcid fluidity, fimilar to that of melting fealing wax. In general the fufion has been accompanied with a flight ebullition, which has fometimes changed the mafs to a kind of froth, and fometimes has merely produced fome fcattered air bubbles. The whole externally and in its fracture fhines much; this fhine, arifing in fome cafes from numberlefs facettes of cryftallization, and in others from a fmooth and continued glofs, like that of glafs. In many fpecimens the cryftallization of newly formed fpar is diftinctly vifible; the cryftalline mafs confifting of parallel plates, which reflect together with one glofs. Some of thefe are difcernible by the naked eye, though in general to fee them well we require the help of a lens. As foon as the carbonate becomes foft, it begins to act powerfully on the tube of porcelain (generally formed of pure Cornifh clay) in which it is confined; the compound fhewing itfelf to be much more fufible than the pure carbonate. It penetrates the minuteft crevices, and fpreads along the cup to a confiderable diftance from the point of contact between the carbonate and the tube; its termination being marked by a black line, the caufe of which I have not difcovered. Previous to this ftage of fufion no action whatever feems to take place between the carbonate and the porcelain, the former receiving from the latter an accurate impreffion of its fhape, acquired doubtlefs when the powder was rammed into the tube. In this cafe the carbonate remains quite loofe, and is often heard to rattle before the veffel is opened. Where pounded filex has been rammed into the tube in contact with the carbonate, an union has fometimes taken place, producing a fubftance having fomewhat the appearance of chalcedony, but which fhews evident proof of fufion, it having flowed fo as to form little ftalactites and ftalagmites. This fubftance effervefces feebly in acid, in fome cafes it leaves a femi-tranfparent cloud of undiffolved matter, in others diffolves entirely the folution, yielding a jelly when evaporated to a certain pitch. This affords proof of a real union between the carbonate and the filex.

Sparry cryftals in the fufed carbonate.

Action of the carbonate on the clay veffel.

Union of the carbonate and filex.

In all the experiments alluded to in this paper, the veffels have been expofed to a violent expanfive force, by which a great number of them have been deftroyed, and the experiments have often been loft or refults obtained only of partial

When the veffels have failed, the carbonate has fhewn different habitudes from lofs of acid.

 fuccefs,

ſucceſs. But theſe have frequently been of value, by bringing into view important collateral facts. Thus I have found that under certain circumſtances, a partial calcination has taken place by the ſeparation of ſome of the carbonic acid from the lime, though enough ſtill remained to preſerve many of the leading properties of a carbonate. When a loſs is ſuſtained, amounting only to two or three, or even four per cent. I find the ſubſtance ſtill ſuſceptible of agglutination and fuſion, but its fuſibility is greatly diminiſhed, a heat of 40 or 50 being required to accompliſh what would have been done in 22 or 25, had the earth continued to be completely ſaturated with carbonic acid, and the carbonate thus obtained is apt to fall to decay by attracting moiſture from the air. Theſe differences afford a good illuſtration of the influence exerted by the acid as a flux on the earth.

—being leſs fuſible, &c.

An apparatus reſembling that of Count Rumford,

Having thus aſcertained the fuſibility of the carbonate under preſſure of indefinite amount, I became deſirous of aſſigning its limits, and of diſcovering the leaſt force neceſſary for this purpoſe. In this view, in addition to my other devices, I followed thoſe uſed by Count Rumford in trying the exploſive ſtrength of gun-powder.*

—and regulated.

By means of a great weight preſſing upon a ſmall opening, and regulated by a counterpoiſe adjuſted at pleaſure, I was able to conſtrain the carbonate to any given amount. In this manner I found that the preſſure of 80 atmoſpheres, anſwering nearly to half a mile of ſea in depth, was requiſite to produce any effect of compreſſion on the carbonate of lime, and that to execute the buſineſs well required a force four or five times greater.

Preſſure 3 or 4 hundred atmoſpheres.

Similar experiments on bituminous matter.

I have likewiſe made ſome experiments with coal treated in the ſame manner as the carbonate of lime, but I have found it much leſs tractable, for the bitumen, when heat is applied to it, tends to eſcape by its ſimple elaſticity, whereas the carbonic acid in marble is in part retained by the chemical force of quick lime. I ſucceeded, however in conſtraining the bitumenous matter of the coal to a certain degree in red heats, ſo as to bring the ſubſtance into a complete fuſion, and to retain its faculty of burning with flame. But I could not accompliſh this in heats capable of agglutinating the carbonate; for I have found, where I rammed them ſucceſſively into the ſame tube, and

* Philoſ. Journal, quarto ſeries, I. 459.

and where the veffel has withftood the expanfive force, that the carbonate has been agglutinated into a good lime-ftone, but that the coal has loft about half its weight, together with its power of giving flame when burnt, remaining in a very compact ftate with a fhining fracture. Although this experiment has not afforded the defired refult, it anfwers another purpofe admirably well. It is known that where a bed of coal is croffed by a dyke of Whinftone, the coal is found in a peculiar ftate in the immediate neighbourhood of the Whin, the fubftance in fuch places being incapable of giving flame, it is diftinguifhed by the name of blind coal. Dr. Hutton has explained this fact by fuppofing that the bituminous matter of the coal has been driven by the local heat of the Whin into places of lefs intenfity, where it would probably be retained by diftillation. Yet the whole muft have been carried on under the action of a preffure capable of conftraining the carbonic acid of the calcareous fpar which occurs frequently in fuch rocks. In the laft mentioned experiment, we have a perfect reprefentation of the natural fact fince the coal has loft its petroleum, whilft the chalk in contact with it has retained its carbonic acid.

Remarkable fact of a production refembling blind coal.

I have made fome experiments of the fame kind with vegetable and animal fubftances. I found their volatility much greater than that of coal, and I was compelled with them to work in heats below rednefs; for even in the loweft red heat they were apt to deftroy the apparatus. The animal fubftance I commonly ufed was horn, and the vegetable faw-duft of fir. The horn was incomparably the moft fufible and volatile of the two. In a very flight heat it was converted into a yellow red fubftance like oil, which penetrated the clay tubes through and through. In thefe experiments I therefore made ufe of tubes of glafs. It was only after a confiderable portion of the fubftance had been feparated from the mafs that the remainder affumed the clear black peculiar to coal. In this way I obtained coal, both from faw-duft and from horn, which yielded a bright flame in burning.

Animal and vegetable matters thus treated.

The mixture of the two produced a fubftance having exactly the fmell of foot or coal tar. I am therefore ftrongly inclined to believe that animal fubftance, as well as vegetable, has contributed towards the formation of our bituminous ftrata. This feems to confirm an opinion advanced by Mr. Keir, which has been mentioned to me fince I made this experiment. I conceive

Probability that coal is of animal as well as vegetable origin.

ceive that the coal which now remains in the world is but a ſmall portion of the organic matter originally depoſited, the moſt volatile parts having been driven off by the action of heat before the temperature had riſen high enough to bring the ſurrounding ſubſtance into fuſion, ſo as to confine the elaſtic fluids and ſubject them to compreſſion.

Horn totally volatilized under ſtrong but not extreme preſſure.

In ſeveral of theſe experiments, I found that when the preſſure was not great, when equal, for inſtance, only to 80 atmoſpheres, that the horn employed was diſſipated entirely, the glaſs tube which had contained it being left almoſt clean, yet undoubtedly if expoſed to heat without compreſſion, and protected from the contact of the atmoſphere, the horn would leave a cinder or coak behind it, of matter wholly devoid of volatility. Here then it would ſeem as if the moderate preſſure, by keeping the elements of the ſubſtance together, had promoted the general volatility, without being ſtrong enough to reſiſt that expanſive force, and thus, that the whole had eſcaped. This reſult, which I ſhould certainly not have foreſeen in theory, may perhaps account for the abſence of coal in ſituations where its preſence might be expected on principles of general analogy.

I have ſhewn ſeveral ſpecimens of theſe reſults to my friends, in particular to Lord Webb Seymour, Mr. Playfair, and Mr. Davy, who have agreed in thinking that the inveſtigation is now brought to ſuch a ſtage of advancement, that the reſult ought to be made public.

I propoſe in the courſe of next winter to lay before the Society a particular account of all theſe reſults, and of the methods followed in obtaining them. In the mean time I ſhall now ſubmit a few of them to the inſpection of the gentlemen preſent.

Exhibition and deſcription of the reſults of carbonate expoſed to heat under ſtrong preſſure.

Nos. 1, 2, 3, 4, 5, 6 and 7, were all produced in ſeparate experiments from pounded carbonate of lime. No. 1, was amongſt the firſt of my ſucceſsful reſults, having been obtained in 1799. It is a firm ſtone, requiring a ſmart blow of a hammer to break it. It was incloſed in a cartridge of paper, the mark of which it ſtill bears. The other ſix are ſtill harder and more compact, approaching nearly in theſe qualities to common lime-ſtone. Nos. 2, 4, and 7, poſſeſs a degree of ſemi-tranſparency moſt remarkable in No. 4, and all of theſe ſpecimens exhibit an uneven fracture, approaching to that of bees-

wax

wax and marble. Their colours are varioufly though flightly tinged with yellow and blue; in particular No. 3, which though produced from common white chalk, refembles a yellow marble. Nos. 3, 5 and 6 have taken a tolerable polifh. No. 7 contains a fhell introduced along with the pounded chalk, and now clofely incorporated with it.

Nos. 8, 9, 10, 11, all formed from pieces of chalk expofed unbroken to heat and preffure. No. 8 is remarkable for a fhining grain and femi-tranfparency. Nos. 9 and 10 fhew parallel planes like internal ftratification which has often appeared in chalk, in confequence of the action of heat, though nothing of the kind could be feen in the native mafs. No. 11, very compact, and of a yellow colour.

Great increafe of denfity and clofenefs.

By various trials, to be given in detail hereafter, it appears that the carbonate in all thefe experiments has undergone a great diminution of bulk, amounting in fome cafes to more than $\frac{1}{3}$ of the original mafs; and that its denfity has been proportionably increafed. At the fame time the porofity of the fubftance has diminifhed in a ftill higher degree. Thus it is found that chalk in its natural ftate abforbs and retains from 20 to 25 per cent. of water; but after being expofed to heat under compreffion, that it does not abforb quite 0.2 per cent. or the 500 part of its weight.

Other fpecimens of bodies expofed to heat under ftrong preffure.

Nos. 12, 13. Examples of welding, in which the pounded chalk has been incorporated with a lump of chalk, upon which it had been rammed, fo that their joining is hardly vifible in the fracture.

Nos. 14, 15, 16. Shewing the fufion of the carbonate well advanced, with a confiderable action on the porcelain tube. In No. 15, the rod of chalk is half melted, and a yellow fubftance produced by a mixture of the carbonate with the porcelain. No. 16 is a lump of chalk, in a ftate indicating foftnefs; a piece of porcelain, which lay in contact with it, having funk a little into the fubftance of the carbonate.

Nos. 17 and 18, being delicate, are inclofed in tubes of glafs. No. 17, formed from pounded chalk, fhews in one part the moft complete formation of fpar with its rhomboidal fracture I have ever obtained. The carbonate having loft fome of its carbonic acid, had crumbled fo much in its effential parts by the action of the air, that the cryftallization was no longer vifible, and I had given up the fpecimen for loft till within thefe

Other ſpecimens of bodies expoſed to heat under ſtrong preſſure.

theſe few days. When employed in examining theſe reſults, a maſs of the carbonate broke in two, and exhibited the fracture now before us nearly in as good a ſtate as it was originally. I immediately incloſed it in a glaſs tube, and ſealed it up with wax, ſo that I have hopes of preſerving it. In the mean time I am happy to ſhew it entire to the Society. No. 18, likewiſe from pounded chalk is perfectly freſh and entire, though made more than a year ago; it ſhews ſome beautiful clear cryſtals of ſpar in parallel plates, but is ſo ſmall as to require the uſe of a glaſs.

Nos. 19, 20, 21, ſhew examples of fuſion and action on the tubes. In number 19, a ſhell is finely united to ſome pounded chalk. In No. 20, the maſs originally of pounded chalk is ſinking upon itſelf, and acting at the ſame time upon the tube. The pure carbonate in its fracture ſhewing brilliant facettes of cryſtallization. In No. 21, the carbonate in a ſtate like the laſt; the compound of porcelain and carbonate ſhewing its liquidity by penetrating the tube ſo as to form a diſtinct vein, and then ſpreading on its outſide to a conſiderable extent, terminating with the black line above alluded to.

Nos. 22, 23, 24, give proofs of entire fuſion. In No. 22, we ſee two porcelain tubes incloſed for preſervation in a glaſs tube, the ſealed end of which muſt be held downwards, to ſhew the poſition in which the experiment was made. The innermoſt porcelain tube ſtands with its muzzle upwards, and the outermoſt covers it in the inverſe poſition; the carbonate was contained in the inner tube. During the action of heat, the barrel failed ſuddenly, and the carbonate has boiled over the lips of the inner tube, running down, as here appears, almoſt to its bottom; thus proving that immediately previous to the failure of the apparatus, the carbonate had been in a liquid ſtate. No. 23, two maſſes of carbonate, welded together in a complete ſtate of froth. The ſubſtance ſhining and tranſparent. No. 24, two ſeparate maſſes expoſed together to heat; one from pounded chalk, now in a ſtate quite like the laſt; the other put in as a lump of chalk dreſſed flat at both ends, and a letter cut on each end (as done in many of the experiments.) It is in a ſhining and almoſt tranſparent ſtate; at one end the flat form and the letter are ſtill viſible; the other end is completely rounded in fuſion, with a gloſſy ſurface.

Nos.

Nos. 25, 26, refults of coal. No. 25, produced by the fufion of common coal under preffure in low red heat. It gave flame powerfully. No. 26, coal produced from horn. It is a fhining black fubftance, exactly refembling pitch or petroleum, and burns with a bright flame.

JAMES HALL.

IX.

Atmofpherical Air not a mechanical Mixture of the Oxigenous and Azotic Gafes, demonftrated from the Specific Gravities of thefe Fluids. In a Letter from Mr. JOHN GOUGH.

To Mr. NICHOLSON.

SIR,

I HAVE already attempted to prove common air not to be a mechanical mixture. The arguments which I ufed for the purpofe, were drawn from the properties of refracted light and the motion of founds through elaftic fluids. Such proofs are of an indirect nature; and though the mathematician may fee the force of them, they may not carry an equal degree of conviction to the chemift, who ought alfo to be convinced. The following obfervations apply more immediately to the fubject; and I do not perceive how they can be refuted, unlefs this be done by difputing the accuracy of the experiments on which they are founded. The prefent enquiry has the recommendation of being ftatical, and the data of my calculations are borrowed from certain experiments made by Mr. Kirwan and M. Lavoifier. According to the former gentleman, if the weight * of atmofpherical air be denoted by 1000, that of an equal bulk of carbonic acid gas will be 1500, of oxigenous gas 1103, of azotic gas 985. An attempt to difcover the comparative weights of a number of gafes is a difficult undertaking; but the preceding ratios may be ufed with fome degree of confidence, becaufe they have been eftablifhed by a philofopher of the higheft reputation. Then in order to apply them to the bufinefs in hand, we will fuppofe the weight of 100 cubic inches of common air to be denoted by unity; though the real weight of the quantity may be ftated at 31 grains,

Introductory remarks.

Experimental data.

Equations to find the oxigen in air.

* Effay on Phlogifton.

grains, according to Mr. Kirwan. Alfo let the characters w, x, and y, exprefs refpectively the cubic inches of the carbonic acid, the oxigenous, and azotic gafes, which compofe the mixture contained in 100 folid inches. Now $w + x + y = 100$ inches by hypothefis. But if 100 inches of air be denoted by unity, the fame bulk of carbonic acid gas will be expreffed by the fraction $\frac{1500}{1000}$, by the experimental data; confequently the weight of this gas in the mixture will be found by the following proportion; *i. e.* as $100 : \frac{1500}{1000} :: w : \frac{1500w}{100000}$. In like manner, the weight of the oxigen is found to be $\frac{1103x}{100000}$, and that of the azote is $\frac{985y}{100000}$. Now the fum of thefe weights is equal to unity by the premifes of the calculation: if then the equation be multiplied by 100000 on the common denominator of the unknown quantities, it affumes the following form; *i. e.* $1500w + 1103x + 985y = 100000$. Thus it appears that we have only two equations, when the quantities to be determined are three; this circumftance leaves the problem unlimited; that is, the value of one of the quantities muft be difcovered without the aid of calculation.

Air contains carbonic acid.

M. Chaptal takes no notice of the carbonic acid in his obfervations on the conftitution of common air; which appears to be an overfight, at leaft in a general view of the fubject; for various proceffes of nature as well as art conftantly difcharge this gafeous acid into the atmofphere, where its prefence is alfo indicated by quick-lime being converted into a carbonate, when placed in open fituations. Thefe facts amount to more than a probability, that the lower parts of the atmofphere contain a flight admixture of the carbonic acid: this fmall portion, however, was undoubtedly retained, in a great meafure, by the azotic gas which Mr. Kirwan ufed in his experiments; becaufe he prepared it from common air, which was confined over mercury, together with a pafte of fulphur and the filings of iron.

x and y determined.

This fingle confideration induces me to follow the example of M. Chaptal in making w of no value; and the ftep may be taken with the greateft fecurity in the prefent inftance, becaufe it increafes the value of x, if it alter it at all; that is, the amount of the oxigenous gas, as found by calculation, will exceed the truth, on the fuppofition that the carbonic acid remained in the azotic gas, which Mr. Kirwan weighed. If, then, w be put equal to nothing, the preceding equations af-

fume

ſume the following forms; *i. e.* $x+y=100$, and $1103x+985y=100000$. Multiply the former by 985, ſubtract the product from the latter, and you will have the following equation; *i. e.* $118x=1500$: Hence by diviſion, $x=12,\frac{42}{59}$; and the exceſs of 100 above this number gives $y=87,\frac{17}{59}$. The ſteps of this proceſs have been detailed minutely, with a view to enable the chemical reader, who has the leaſt knowledge of algebra, to conſider the grounds of the following concluſions, and to form his own judgment reſpecting the weight of them.

Air not a mixture of oxigenous and azotic gaſes.

It is evident from the foregoing calculation, that if 100 parts of a mixture of the oxigenous and azotic gaſes, having the ſpecific gravity of common air, were deprived of their oxigen in a graduated tube, the reſiduum would meaſure ſomething more than 87 ſuch parts, *i. e.* $87,\frac{17}{59}$. But when an equal bulk of the atmoſpherical fluid is treated in the ſame manner, the portion of it which remains unabſorbed, is much leſs than the preceding quantity; M. Chaptal makes it to be 72 ſuch parts, and ſome writers call it 78, the mean of which is 75. Now if we ſubtract any one of theſe numbers from the reſiduum fixed by calculation, a difference will be found, which cannot be referred to the unavoidable imperfections of eudiometers, becauſe it could hardly eſcape obſervation in any inſtrument of the kind, the leaſt exceſs being more than nine parts, or nearly a tenth, of the whole ſcale of 100 parts. On the other hand, a mixture of 72 parts azote and 28 oxigen, or of 78 of the former and 22 of the latter, exceeds an equal bulk of common air in weight; conſequently the atmoſpherical fluid is not a mechanical mixture of the two gaſes in queſtion, if any credit be due to the experimental data.

Air a gaſeous oxide of azote.

Though air has been ſhewn not to conſiſt of the oxigenous and azotic gaſes ſimply mixed together, it is certainly a compound that may be reſolved into theſe two principles: For beſides ſupporting reſpiration and combuſtion, it converts metals into oxides, and the nitrous gas into nitric acid; therefore it contains the oxigenating principle. On the other hand, when air is employed to oxigenate bodies, the reſiduum of it is azotic gas of greater or leſs purity; conſequently the atmoſpherical fluid is a gaſeous oxide of azote, which can be decompoſed by art, though chemiſts have not as yet diſcovered a certain method of producing it at pleaſure, by uniting the oxigenating matter to the azotic baſe.

Statical analyſis of air.

Facts have obliged me to give a name to common air, which has been hitherto excluſively applied to the dephlogiſticated nitrous air of Dr. Prieſtly. This remark being made for the ſake of perſpicuity, I will endeavour, in the next place, to give a ſtatical analyſis of the atmoſpherical fluid. If 100 cubic inches of common air weigh 31 grains, the weight of an equal bulk of azotic gas will be 30.535 grains; becauſe as 1000 : 31 : : 985 : 30.535 : in like manner the weight of 100 cubic inches of oxigenous gas will be found to be 34.193 grains. The quantity of azotic gas in 100 cubic inches of air, will be ſtated at 75 inches in the preſent calculation, for the following reaſons: *Firſt*, Becauſe 75 is the mean of 72 and 78; *ſecond*, Becauſe M. Lavoiſier found, that four inches of oxigenous gas and 16 inches of air ſaturate equal quantities of the nitrous gas. Theſe premiſes being ſettled, we ſhall find the weight of 75 inches of azotic gas to be 22.90125 grains; conſequently the weight of the oxigen gas in 100 cubic inches of common air, is the exceſs of 31 above the laſt number, or 8.09875 grains; therefore as 34.193 : 100 inches : : 8.09875 grains : 23.685 inches, which is the meaſure of the oxigen gas in 100 cubic inches of common air, when the azotic gas is ſtated at 75 inches. Thus it appears, that if 100 parts of the atmoſpherical fluid were decompoſed, the elementary gaſes would occupy together no more than 98.685 ſuch parts; and a difference of a like nature will be obſerved, if the azotic gas be called 72 or 78 *per cent.*; hence it follows that the denſity of air is leſs than that of the mechanical mixture of its elements. This poſition may appear paradoxical at the firſt view, but chemiſtry can furniſh various inſtances of compounds, which are ſpecifically lighter than the aggregates of their ingredients.

The oxigen of the air compared with the oxigenous gas.

This analyſis muſt remain incomplete, until the powers of the oxigen of the atmoſphere have been compared with the correſponding effect of an equal weight of the oxigenous gas. Such an attempt, however, is liable to great uncertainty; becauſe the experiments which ſhould ſupply the neceſſary data, are variouſly repreſented by different writers. M. Lavoiſier ſays, that four parts of oxigenous gas and nearly 16 of common air, oxigenate equal bulks of nitrous gas, namely $17\frac{1}{3}$ parts. On the contrary, M. Chaptal found by repeated experiments, that 12, or at moſt 13, parts of air were ſufficient

cient to ſaturate the ſame quantity of nitrous gas. The data of the following calculation are taken partly from the one and partly from the other author: I have ſuppoſed with M. Lavoiſier, that three inches of the oxigenous will acidify 13 inches of the nitrous gas: M. Chaptal is followed in other reſpects, namely, the atmoſphere is imagined to contain 72 *per cent.* of azote, and 39 inches of air are made equivalent to 52 of nitrous gas. The calculation, which is formed upon theſe ſuppoſitions, will in all probability prove incorrect; but I have ventured to inſert it, as being a novelty, which is likely to excite enquiry. If 39 inches of air can ſaturate 52 of nitrous gas, 100 inches of the former require 133.333 of the latter. Again, if 13 inches of nitrous gas demand three of oxigen, 133.333 of the former will require 30.303 of the latter. But the weight of 30.303 inches of oxigen gas amounts to 10.361 grains, which is equivalent in effect to 100 inches of air. Now 72 inches of azotic gas weigh 21.985 grains, which being taken from 31 grains, leaves 9.015 grains for the weight of the oxigenous part of 100 inches of air; in round numbers, 90 grains of the latter kind of oxigen are equal in effect to 103 of the former.

I know that M. Lavoiſier, in ſpeaking of the conſtitution of nitric acid, makes 100 grains of it contain 64 grains of the nitrous and 36 of the oxigenous gas; or 173 inches of the former and 105 of the latter. Had the data of the preceding calculation been taken from this ratio, the comparative ſuperiority of atmoſpherical oxigen would have been much greater than it appears to be by the laſt paragraph. A preference, however, has been given to the preceding hypotheſis, becauſe I deſire to excite enquiry, rather than expect to eſtabliſh any thing of a permanent nature concerning the ſubject.

The preceding arguments, in conjunction with others of a kind more uncertain than themſelves, ſuggeſted to me the probability of atmoſpherical oxigen poſſeſſing a greater degree of efficacy than an equal weight of vital air. This idea occurred to me ſeveral months ago; and I at length reſolved to propoſe it to the public in a number of hypothetical queſtions, which appeared in your Journal for Auguſt. Theſe queries are evidently borrowed from ſome ingenious ſpeculations, that were publiſhed in the eighth volume of the ſame work *; for

The origin of the queries in the Phil. Jour. vol. viii. p. 246.

* Page 88.

the

the queries imply, in common with the remarks here alluded to, that the oxigenating matter is water chemically united to the positive power of the galvanic pile. In order to accommodate this notion to the case of common air, I have supposed that the oxigen of the atmosphere receives a stronger charge of the power in question, than that which is imparted to vital air; in consequence of which it takes up an additional quantity of water, upon being disunited from the azotic basis, and is thus converted into oxigenous gas. Should future experiments discover that bodies, which are oxigenated by given portions of common air in contact with water, acquire more weight than the air loses, the discovery will undoubtedly open a wide field for enquiry.

JOHN GOUGH.

Middleshaw, Sept. 5, 1804.

X.

Letter from THOMAS YOUNG, *M. D. F. R. S. &c. announcing the Discovery of a new moving Star, by Mr.* HARDING, *of Lilienthal; and on other Subjects.*

To Mr. NICHOLSON.

DEAR SIR,

New planet lately discovered.

I HAVE just received a letter from Dr. Gauss of Brunswick, F. R. S. dated September 11, in which he informs me that, a few days before, Mr. Harding at Lilienthal had discovered "a new moving star, most probably another new planet of our solar system." Dr. Gauss is certainly a person on whose judgment much dependance may be placed: He has sent some further particulars of the discovery to the Astronomer Royal.

Maximum of density of water.

I take this opportunity of making a criticism on a work of great experimental merit which has lately appeared. It relates to the apparent expansion of water in a vessel of glass. Mr. Leslie calculates, from the supposition that the expansion of water is proportional to the square of the degrees of temperature above the freezing point, and from the expansion of glass measured by General Roy, that the apparent maximum of

of denſity ſhould be at 6° of the decimal ſcale: but he has taken in this calculation the mean rate of expanſion from the freezing to the boiling point, inſtead of the expanſion at the boiling point, which, upon the ſuppoſition, is twice as great; hence the apparent maximum, on the ſame grounds of calculation, ſhould be at 3° of the decimal ſcale, or at 37.4° of Fahrenheit. Mr. Dalton attempts to avoid this difficulty by ſuppoſing that the thin bulb of a thermometer expands more than the glaſs tube employed by General Roy.

The ſame ingenious author has made experiments on the elaſticity of different ſubſtances, by meaſuring the depreſſion of the middle of a bar ſupported at its extremities; but if I am not much miſtaken, his inferences from them are by no means accurate. Mr. Leſlie gives 671625 feet for the height of a column of deal equivalent to its elaſticity; the true height reſulting from his experiments, taking into conſideration alſo the inequality of curvature, appears to me to be 4664000 feet; which is ſtill little more than half as much as would be inferred from the experiments of Chladni on the longitudinal ſounds of fir wood. There muſt have been ſome inaccuracy in Mr. Leſlie's experiments on ſteel differently tempered: for it appears from the direct experiments of Coulomb on the flexure of bars, as well as from thoſe of Chladni, and ſome of my own, upon ſound, that the ultimate elaſticity of ſteel in ſmall tenſions is the ſame, whether it is harder or ſofter. This may appear at firſt ſight paradoxical, but it admits of a ſufficient explanation, which, together with many other illuſtrations of various parts of natural philoſophy, will probably before long be laid before the public. Elaſticity of bodies.

Your very obedient ſervant,

THOMAS YOUNG.

Welbeck Street, Sep. 22, 1804.

XI.

Defcription of an Inftrument to afcertain the Number of Lifts made from a Mine, in any given Time. By Mr. John Antis *.

SIR,

Inftrument for counting the lifts from a mine.

I HAVE lately been encouraged by fome gentlemen engaged in fome coal mines in this neighbourhood, to invent a machine, which would infallibly tell the number of boxes of coals drawn out of a pit in the courfe of a week, or any given time. I have juft now completed a model, which I think would anfwer the purpofe extremely well.

Before, however, I make it known, I thought I would inquire of you, whether fuch an invention might be of a more extenfive ufe, and as fuch would deferve the attention of the Society.

The machine is very fimple, and need not be expenfive; its properties are as follow :

1. It is of no confequence if the pit be ten, twenty, forty, or more yards deep. It will, notwithftanding this difference, only point out one box at once; and, fuppofing the pit to be feveral hundred yards deep, the principle will remain the fame, and the machine could be eafily adapted for it.

2. It is likewife of no confequence if the coals be drawn up by a hand windlafs, or any other machine turned by horfes or other powers.

3. No account is neceffary to be kept for a whole week, or any given time, as the machine can be calculated for any quantity of coals whatever, that can be drawn up in a given time. Only allowance muft be made for the number of perfons that go down and up in the fame way.

I have not yet heard of any contrivance of this kind; and therefore, if there be any fuch, it is unknown to me. Whoever knows the fraud, which is but too often committed by bankfmen, in accounting for the quantity of coals procured from a pit, will readily admit the utility of fuch a contrivance. And it may moft likely alfo be ufeful in tin, lead, and other

* From the Tranfactions of the Society of Arts, 1803.

mines.

mines. Should the Society be of the ſame opinion, then I will ſend you a ſtill more complete model than at preſent. Your ſpeedy anſwer will very much oblige

Inſtrument for counting the lifts from a mine.

Your moſt obedient Servant,

JOHN ANTIS.

Fulneck, March 6, 1802.

Charles Taylor, Eſq.

SIR,

I HEREWITH ſend you a model of my machine, which I have before mentioned to you, for pointing out any number of boxes or baſkets of coals, or other minerals, which are drawn out of a pit in any given time. I ſhall be much obliged to you, if you will lay it before the Society for the Encouragement of Arts, &c. for their inſpection, and ſhall be glad that they find it of public utility.

It will eaſily be perceived, that by enlarging the wheels and multiplying the cogs of them, the number of yards required for the depth of a mine, as likewiſe the number of boxes which are wanted to be pointed out may be carried to any extent.

I am ſo little acquainted with coal-pits, or any other mines, that I do not know the technical terms by which the different parts are called, nor all the methods now uſed to draw up thoſe materials; I muſt therefore beg the indulgence of the Society, if I do not always expreſs myſelf with accuracy.

By inventing this machine, I aimed at aſcertaining the number of boxes, my ideas reſpecting which I will endeavour to deſcribe, to the beſt of my abilities.

Firſt, the rope muſt be ſo long, that it can be faſtened at its middle to the windlaſs, and that each end thereof may reach to the bottom of the pit. This precaution will not be neceſſary, if it can be prevented by any other means from ſhifting or ſliding, which I leave to others, more acquainted than I am with the preſent practice, to effect.

The roller A, *Plate* VII. *Fig*. 1, in the model, with the endleſs ſcrew, repreſents the gudgeon in full ſize of a hand windlaſs, ſuch as are uſed in this neighbourhood, particularly where the pits are of conſiderable depth. As no great ſtreſs is laid upon this ſcrew, it may be made, in order to ſave labour, of braſs,

 and

Inftrument for counting the lifts from a mine.

and driven, or otherwife faftened, upon any gudgeon. I fuppofe the windlafs to be one yard in circumference; and confequently forty revolutions of the windlafs are required before the firft wheel B, upon which the brafs catch C is fixed, turns once round. The pit, therefore, may be only ten yards, or from feventy to feventy-five yards deep, yet it will make no difference. All that is required to regulate the machine to the depth of the pit, is as follows:—If the pit be little more or lefs than ten yards, the firft wheel muft be fo placed in the endlefs fcrew, that the brafs catch C may be at the beginning, but juft efcaped from the cogs of the fecond wheel D. It will then only want five or fix revolutions of the windlafs backwards, before the faid catch is ready to operate again. But fuppofing the pit to be feventy yards deep, in this cafe, the catch C muft be placed fo as nearly to be ready to act. By turning backwards, the wheel may very nearly make two revolutions; and as it can be ftill turned as far the other way, the catch will only act once, provided the faid wheel does not quite make two revolutions.

The model is about the fize required for a common hand windlafs. The index will at once fhow how many times fifty boxes, and how many above and below that number, have been drawn up: not one of them can efcape. Allowance, however, muft be made for the number of perfons who go up and down in the fame way. Befides this, fince one box is always coming up, whilft the other is going down, the numbers pointed at muft be doubled, except a perfon was defirous to have a machine acting both ways, which would hardly be worth while. As the machine is at prefent, though it points out a thoufand boxes only, yet it will ferve for two thoufand.

The fame mechanifm, with very little variation, may be fixed to a horfe machine, called a gin, in this neighbourhood; viz. as a gin may be ten yards or more in circumference, the action would be too flow, if moved by an endlefs fcrew; a pinion, with four leaves, will be found much more convenient. This pinion is fixed to the end of the upper gudgeon, which enters the box containing the machinery. As fome accuracy is required to make fuch a pinion work well, and thefe machines are often coarfely conftructed, it will be neceffary only to make about one inch and a half of the end of the faid gudgeon exactly round, namely, that part of it which goes through the

bottom

bottom of the box, and then faſten the latter ſo, that it can give way to any remaining inaccuracy of the other parts of the gudgeon.

Inſtrument for counting the lifts from a mine.

I have ſent a drawing * of ſuch a machine, which will be eaſily underſtood, ſince the principle is the ſame as in the model. But as gins are commonly made uſe of in deep mines, I have made it upon a larger ſcale; viz. the firſt wheel has fixty-four cogs; and this, with a pinion of four leaves, will require ſixteen revolutions of the gin, for one of the wheel. Allowing ten yards for the circumference of the gin, it will make one hundred and ſixty yards of rope; and conſidering that the ſaid wheel can nearly make two revolutions, and that the catch will ſtill act only once, it will conſequently ſerve for a pit of three hundred yards deep, as well as for one of ten or twenty.

In this machine, the three wheels are placed one on another, and the index is divided; viz. the fifty on the ſecond, and the 1000 on the third wheel. As the wheels move, the two hands are ſtanding, which will point out the numbers as exactly as thoſe in the model. Indeed, machines for hand windlaſſes may be made in the very ſame way, and thereby be more ſimple, though the difference is but ſmall; viz. the box would require but one lid and lock, and the two ſmaller wheels become unneceſſary.

Though in ſetting the machine ſo as to begin counting with one, it may eaſily be effected by lifting out the ſpring, and then turning the wheel with the other hand, till both hands ſtand upon one, yet, in the double index, the upper hand muſt only be ſet thus with the wheel, and the lower may be moved round on its ſocket.

In my humble opinion, my machines may be uſeful in two ways; firſt, to the honeſt banksman, as they will ſave him the trouble of noting down each box which is drawn up, and prevent miſtakes; ſecondly, in the prevention of fraud, which will be their greateſt and moſt important uſe. To obtain this end, it is neceſſary that the machine be ſo contrived, that nothing eſſentially wanted can be diſengaged. I have therefore made the model ſo as to fruſtrate any ſuch deſign. All is within the box; and neither the endleſs ſcrew can be diſen-

* This drawing is in the Society's poſſeſſion.

gaged

Inftrument for counting the lifts from a mine.

gaged from the wheel, nor the fpring; nor can the number on the index be altered without firft opening the box. This latter can be made of iron, if found neceffary, with a good lock to it; otherwife, the ftrefs is fo very little, that even one of wood, made in all refpects like the model, would anfwer, if it could be fecured from the effects of the weather. In the machine likewife, reprefented by the drawing, nothing can be altered, except the box be firft opened. This machine is faftened to the beam in which the upper gudgeon turns. To effect this, the bottom of the box muft have two projecting ends with holes in them, larger than neceffary to receive a ftrong pin or rivet; which pins muft have large heads, fo that when properly driven or riveted in, the box may be able to give way to any remaining inaccuracy in the lower parts of the gudgeon, as I before obferved. Thofe machines for hand windlaffes, like the model, are fixed to the fide of the poft into which one of the gudgeons turns; but perhaps thefe might likewife be contrived fo, that the gudgeon with the endlefs fcrew may be a piece by itfelf, attached only to the windlafs by a fquare, or otherwife, as is often practifed in other machineries, fuch as cotton-mills, &c.

I am, Sir,

Your obedient Servant,

JOHN ANTIS.

Fulneck, May 18, 1802.

Charles Taylor, Efq.

Reference to the Model fent by Mr. ANTIS *to the Society.*

Plate VII. *Fig.* 1. A. The roller, with an endlefs fcrew, to be the fize of the gudgeon of a hand windlafs.

B. The firft wheel, of forty teeth, with a brafs catch C. It may be moved backwards or forwards by the endlefs fcrew.

D. A ratchet wheel with fifty teeth, which, when the wheel B is put in motion one way, the catch will flide over the teeth of it; but, on turning it the other way, the catch will drop into and forward this wheel one tooth, in which fituation it is prevented from returning by the fpring E, placed on the fide of the box which holds a tooth on the fide of the wheel.

F. A fmall pinion of four teeth, on the back part of the axle of the wheel D. This pinion moves a large wheel on the other fide of the box.

Fig.

Fig. 2. G. The large wheel laſt mentioned: it has eighty teeth.

Inſtrument for counting the lifts from a mine.

H. A ſmaller wheel of fifty teeth, fixed faſt on the face of the wheel G; which wheel H works a wheel I, of the ſame number of teeth, ſhown by dotted lines behind the figured dial-plate.

K. The ſhort hand, or finger of the dial, is placed upon an arbor or collar of this wheel I, and moves with it.

The long finger, or hand of the dial-plate, goes upon a ſquare on the end of the ſpindle on which the above arbor moves; which ſpindle moves forward this long finger one diviſion in fifty of the outer circle marked on the dial-plate, every time the rope, or any thing attached to it, has been down to the bottom of the mine, and returned to the top.

The inner circle of the face of the dial is marked in diviſions of fifty each. When therefore fifty draughts up and down have been made from the mine, the ſhorter hand will be found, if both hands were originally ſet correctly at the top, to have moved to the firſt interior diviſion marked 50; and ſo on, in proportion, will advance as more draughts are made.

K, Is a board placed under the wheels B D, in *Fig.* 1, and which ſeparates them from the other wheel-work in *Fig.* 2, where only a few teeth of the wheel B appear behind it.

L L, Show the temporary handles in both figures; and the poſition of thoſe letters denote that the gudgeon of the windlaſs, when the machine is in actual uſe, ſhould be there joined to, or make a part of the roller A.

Fig. 3, Shows, on an enlarged ſcale, the form of the catch C. The ſteel ſpring M preſſing againſt the pin N, returns the catch to the tooth of the wheel D, when it has been forced back; and a pin fixed underneath the catch moves in a groove O, made in the wheel B, to prevent the catch being preſſed too far back, or thrown out improperly by the ſpring.

A door is fixed on each ſide the box, and ſhould be locked, to prevent the hands being altered unknowingly.

The drawing of a machine for ſimilar purpoſes, intended to be uſed with a gin or horſe-wheel, alluded to in Mr. Antis's letter, is in the Society's poſſeſſion, if a reference thereto be thought neceſſary.

On

XII.

On Galvanifm. In a Letter from RA. THICKNESSE, *Efq.* To Mr. NICHOLSON.

SIR, *Wigan, Sept.* 20, 1804.

On the caufes which retard difcovery.

ALTHOUGH the production of the electric fluid by the galvanic pile has never yet been fatisfactorily accounted for, it appears to me fomewhat eafy to be explained, from a confideration of the principles of chemiftry. This, I am aware, is a very bold opinion from a man who is a mere dabbler in philofophy; but as it muft be allowed that the ableft philofophical inquirers and experimentalifts have been guilty of extraordinary overfights, I truft my prefumption is excufable. When an ardent mind has once entered a wrong path in purfuit of knowledge, it is too intent upon the objects before it to turn afide, and too anxious to proceed to be induced to look back. Thus chemifts fifty years ago thought they obtained earth from water, becaufe they omitted to weigh the veffels in which they made their experiments; and thus, perhaps, the profeffors of galvanifm at the prefent day, being of opinion, as I believe they all are, that the electric fluid proceeds from the metals, devote little of their attention to the fluid employed in the pile, confidering it almoft as a mere conductor. Nothing can be more obvious to a chemift now than the neceffity of weighing the matter he fubjects to experiment, and alfo the product; yet the difcovery alone of this neceffity overturned the opinions, and falfified the wifdom, of all the previous cultivators of the fcience, and led to the modern theory of chemiftry and its improvements; an omiffion how trifling in itfelf to keep men (philofophers too) poking in the dark for ages!

Argument or inference that water is compofed of oxigen, hidrogen, and electricity.

Water, we are told, is compofed of certain parts of oxigen and hidrogen; but, as to form thefe bodies, from a ftate of gas at leaft, into water, it is neceffary to pafs an electric fpark through, or rather *into* them (no matter whether the procefs of formation be combuftion or not), and as water owes its fluidity to heat, the matter of heat and the electric fluid being probably the fame, or modifications of the fame body, I think it reafonable to affume that water is compofed of oxigen, hydrogen, and *the electric fluid.*

The

The experiments by which M. Volta and others have endeavoured to ſhew that piles compoſed of metals only afford the galvanic fluid, are ſo little demonſtrative of it, that they ſeem to me equally illuſtrative of the contrary; and I believe that all galvanic piles muſt, with the intervention of fluids, be compoſed of two metals, or *other ſubſtances,* which have an affinity, the one for oxigen and the other for hidrogen; for water and other fluids made uſe of in galvanic piles, are decompoſed; and not, I conceive, as hath generally been imagined, by the electricity of the metals; but by the action of affinities. For inſtance, in a pile formed with zinc, copper, and water, the oxigen and hidrogen of the water having a ſtronger affinity for the zinc and copper (the oxigen for the zinc and the hidrogen for the copper) than for each other, unite with them, the water being conſequently decompoſed, and the electric or galvanic fluid, which was contained in it, conſequently ſet at liberty.

Decompoſition of fluids in galvaniſm aſcribed, not to the action of electricity, but to chemical affinity.

In ſupport of this theory I may alledge as facts, that the firſt ſhock from a galvanic pile is generally the ſtrongeſt, when the energy of the affinities is the greateſt; that for a repetition ſome interval of time is requiſite, whilſt the decompoſition is taking place; that the metals in the pile act, not according to their quantities, but according to their ſurfaces—the greater theſe are, the greater being the quantity of water acted upon; that the zinc is always oxidated, and the copper (or ſilver) always acted upon by the hidrogen, being rendered more brittle, &c.; that any alteration in the arrangement of the pile, which brings two pieces of the ſame metal to the ſame ſtratum of water, inſtead of one piece of each metal (one to act on the oxigen and the other on the hidrogen), interrupts the proceſs, and, no decompoſition taking place, there is no electric fluid produced; and it may alſo be added, that this laſt is a fact which cannot otherwiſe be accounted for.

Fuller explanation from the facts.

I am not ignorant that a ſtream of the galvanic fluid from the pile, paſſed through water, decompoſes it; but it is no proof, I preſume, that water contains none of this fluid, becauſe it is decompoſed by a greater quantity than will chemically mix with it. If we pour a ſmall quantity of water impregnated with carbonic acid gas into lime-water, the lime is precipitated, or *compoſed,* the lime-water becoming turbid; but

Objection, that the galvanic ſtream decompoſes water, anſwered.

but if we add a further quantity, the lime is again decompoſed, and the lime-water becomes again perfectly clear.

I am, Sir,

Your moſt obedient ſervant,

RA. THICKNESSE.

XIII.

Deſcription of an Inſtrument for drawing in true Perſpective from Nature, and of another of conſiderable Simplicity and Cheapneſs for delineating Ovals. In a Letter from a Correſpondent, R. B.

To Mr. NICHOLSON.

SIR,

Introduction.

AS I obſerve that you are willing, in your capacity of Journaliſt, to lay before the public any ſketch or outline of invention that may promiſe to be uſeful, whether in its ultimate ſtate of improvement or not, I am encouraged now and then to ſend my thoughts, queries, obſervations, or news, as they may occur. The following inſtruments are offered to your notice, in hopes they may appear in your excellent collection.

Inſtrument for drawing in perſpective.

Fig. 1, *Plate* VIII. is a ſketch of an inſtrument for perſpective, made ſome years ago by Dolland, and of which I know not the inventor. A teleſcope or camera is ſuſpended vertically on a frame by an univerſal joint or jimbals. Horizontal rays A, are directed down the tube by a plane mirror B, and are again rendered horizontal, and turned to the eye through a ſide hole in the tube, by another mirror C. At the lower end is a pencil E ſliding in a well-fitted ſocket, and preſſed gently downwards by a weight or ſpring; or ſtill better, by the hand only. The reſult or uſe is, that while the images are in ſucceſſion brought into apparent contact with a point in the field of view, the pencil may be employed in tracing them in true perſpective upon the table beneath *.

* There is an omiſſion of the grey or rough glaſs, if the drawing be meant for a camera; or of the eye-piece, if it be a teleſcope. The firſt focal convergence muſt be made in theſe, and not at the eye.—N.

5 *Fig.*

Fig. 2 reprefents a fimple rule and ftring for drawing ovals on paper. A C B is a filken thread, fixed at A, and capable of being lengthened, fhortened, and fixed by a fcrew B at the other end. This fcrew B can be placed, by a longitudinal groove in the ruler, at any diftance from A, and can be made to pinch the thread upon any one of the divifions of the rule. At C is a pencil to be moved in the bend of the thread. It muft be held upright, and it would be eafy to contrive means of keeping it fo; but it does not feem an object of fufficient neceffity to add to the price of the inftrument. Inftrument for drawing ovals.

In the ufe, fet A at one focus of the intended oval and B at the other. Allow the ftring to extend till the pencil marks the extremity of the conjugate diameter. Draw the femi-oval by moving the pencil along in the ftretched thread: Then reverfe the points A and B, placing them refpectively on the foci occupied before by each other. Draw the other femi-oval, which completes the figure.

I am, Sir,

Your obliged correfpondent,

R. B.

XIV.

On the Computation of Tables of Squares and Cubes. In a Letter from H. G.

To Mr. NICHOLSON.

SIR, *Sept.* 8, 1804.

AS I can employ my time in a more ufeful, pleafant, and advantageous way than by making tables of any kind, I have no defire on my own account to trouble your readers with the following remarks, and fhall therefore leave it entirely to your choice to notice them or not in your Journal. Introduction.

I am, Sir,

Your humble fervant,

H. G.

YOUR correfpondent E. O. in his note, p. 79 of your Journal for this month, has very properly pointed out an error in On the computation of tables of fquares and cubes.

On the computation of tables of squares and cubes.

in the rule laid down by me in p. 150 of your Journal for July last, which will be corrected by substituting the word *given* for the word *next*.

But he does not seem to be aware that the rule I proposed for finding the squares of roots in an arithmetical progression of those roots, is precisely the same as that he has used and elucidated in pages 5 and 6 of your Journal for this month.

Neither has he made me a convert to his doctrine, that addition should be used in preference to subtraction; for the one appears to me as easy an operation as the other, for all the purposes to which it is applied in the construction of the tables in question.

With respect to the construction of the table of cubes, I will barely state my method of computing the cubes of the same roots E. O. has calculated, in pages 8 and 9 of your Journal for this month, and will leave it to those who may be inclinable to enter on the investigation, to determine which method will be most advantageously practised, his or mine, only observing, that E. O. has a continual repetition of the first differences, which I have entirely avoided without adding any figures, in other respects, to those he has used.

I still think my method of computing the table of cubes is not more liable to inaccuracy than that E. O. has adopted; but in order to avoid almost the possibility of error in my computation, and at the same time to add to, examine, and correct Mr. Councer's table, I would place the cubes of half the given roots × 8. at such distances under each other, as would enable me to interpolate my calculations of the cubes of twice those roots, as it is done in the left hand part of the following elucidation; and then it is evident, every other cube obtained by the calculation must be the same as that obtained from the table; and thus a proof of the accuracy of Mr. Councer's table, and of the continuation of it, would go hand in hand.

TABLE.

TABLE,

H. G.'s Method to obtain the Cubes of certain Roots.				Supposed Mr. Councer's.		Cubes of H. G.'s Roots *per contra*, or
	Cubes.	Roots.		Roots.	Cubes.	Cubes of twice the Roots opposite.
$a =$	$26560 =$	18736316416000	$= A$ - - - - - -	13280^3	$= 2342039552000 \times 8 =$	18736316416000
$b =$	$26559 =$	18734200194879	$= B$			
		2116221121	$= C = A - B$			
		159360	$= D = 6a$			
$c =$	$26561 =$	18738432796481	$= E = D + C + A$			
		2116380481	$= F = E - A$			
		159366	$= G =$			
$d =$	$26562 =$	18740549336328	$= H = G + F + E$ -	13281^3	$= 2342568667041 \times 8 =$	18740549336328
		2116539847	$= I = H - E$			
		159372	$= K = G + 6 = 6d$			
$e =$	$26563 =$	18742666035547	$= L = K + I + H$			
		2116699219	$= M = L - H$			
		159378	$= N = K + 6 = 6e$			
$f =$	$26564 =$	18744782894144	$= O = N + M + L$ -	13282^3	$= 2343097861768 \times 8 =$	18744782894144
		2116858597	$= P = O - L$			
		159384	$= Q = N + 6 = 6f$			
$g =$	$26565 =$	18746999912125	$= R = Q + P + O$			

Letter

XV.

Letter to the Editor from Mr. William Henry, *in reply to Mr. Gough.*

Manchester, September 13, 1804.

SIR,

Previous remarks.

NOTHING was farther from my intention, when I communicated to you the "Illustrations of Mr. Dalton's Theory of Mixed Gases," than to enter into controversy respecting a doctrine, to the defence of which I may naturally be supposed to be much less competent than its author. Yet it is certainly required of me, both by the respectful attention due to your correspondent Mr. Gough, and to others of your readers, either to explain and support, or to relinquish, if erroneous, the opinions respecting which I have publicly expressed a coincidence with Mr. Dalton. The last alternative I do not, at present, feel disposed to adopt, because I am far from being convinced by Mr. G.'s reasoning; and in the explanation, which I am about to offer, I shall confine myself to those proofs of the new theory of mixed gases, which are furnished by my own experiments; leaving to Mr. Dalton the more important office of establishing its fundamental evidences.

The quantity of gas absorbed being as the pressure, the last is taken to be the cause,

It is by no means clear to me, whether or not Mr. Gough denies the principle, "that the relation between gases and water is altogether a mechanical one."* To me this appears as legitimate an inference, as can possibly be deduced in physics; for the quantity of every gas, absorbed by water, follows exactly the ratio of the pressure: And, since it is a rule in philosophizing, that effects of the same kind, though differing in degree, are produced by the same cause, it is perfectly safe to conclude, that every, even the minutest portion of any gas, in a state of absorption by water, is retained entirely by incumbent pressure. There is no occasion, therefore, to call in the aid of the law of chemical affinity, when a mechanical law fully and satisfactorily explains the appearances. And when the effect ceases, it is equally conformable to just reasoning to infer, that this happens, in every case, solely in consequence

* Certain acid gases, the muriatic for instance, are obviously excluded.

of

of the relaxation or removal of that mechanical power, which held the gas in its situation. Under all circumſtances, therefore, when a gas eſcapes from water, whether by placing the aqueous mixture under an exhauſted receiver, or in an atmoſphere of a different gas, the cauſe operating its eſcape, muſt be one and identical, viz. the diminution of mechanical preſſure. Before we account for any effect by compariſon of affinities, the affinities themſelves ſhould be proved to exiſt. But with reſpect to the relation of gaſes to water, the proof fails in every inſtance; for how can that effect be fairly aſcribed to chemical affinity which is deſtroyed, as is the connection of every gas with water, by an unmixed mechanical cauſe; and is it not abſurd to compare powers which have no exiſtence in nature?

—and the preſſure is of no other gas than that abſorbed.

Admitting then the connection between gaſes and water to be entirely dependent on phyſical preſſure, there naturally ariſes out of this law an explanation of the curious fact, which I have aſcertained, that each gas, when abſorbed by water, is retained in its place by an atmoſphere of no other gas but of its own kind. Under any other atmoſphere, the abſorbed gas eſcapes, even without agitation, though this certainly accelerates the event. Now the ſubaqueous gas can only accompliſh its change of place by virtue of ſome active principle or power inherent in it, and cauſing its movement; and this power is its elaſticity, which is not counteracted by that of an incumbent gas of a different ſort. The fact affords, therefore, ſomething further than "probability," that the particles of gaſes preſs only on thoſe of their own kind; for to ſay that the elaſticity of the ſubaqueous gas is not counteracted by any incumbent one of a different ſort, is to aſſert in other terms, that the one is not preſſed by the other,—the principle which I am ſolicitous to eſtabliſh.

The theory is an induction from facts; capable of mathematical treatment, but founded on obſervation.

The above remarks are the only ones which I deem it within my province to urge in reply to Mr. Gough. Before cloſing this letter, however, I muſt exculpate myſelf from the charge of attempting to uphold a mechanical theory, by probabilities drawn from chemical facts. This ſtatement is not correct; the facts which I have alledged are purely ſtatical, and my object has been to prove that they were before erroneouſly included under the laws of chemical affinity, with which, in reality they have no connection. Theſe mechanical phenomena, I have

have brought in ſupport of the general principle "that the particles of gaſes preſs only on thoſe of their own kind;" and in ſo applying them, I truſt I have conformed to the rigorous laws of philoſophical induction. Such general principles are not uncommon in natural ſcience; and though, like the law of gravitation and ſome other leſs comprehenſive ones, they may be mathematically purſued and inveſtigated, yet they are not derived from mathematical reaſoning, but from a method of enquiry which Newton himſelf did not diſdain to employ,—that of induction on the baſis of experiment and obſervation. The ſpeculations of Mr. Dalton, being founded on well aſcertained facts, appear therefore to me to be fairly entitled to the appellation of *theory*, and not to be included within the definition of *hypotheſis*,* handed down to us from the father of experimental philoſophy. I remain, very truly, Sir,

Your's, &c.

WILLIAM HENRY.

XVI.

Deſcription of a very ſimple and cheap Contrivance for making Port Folios of large Dimenſions. By the late JAMES MALTON, *Eſq.*†

SIR,

Method of conſtructing large port-folios.

AS I well know the great inconvenience experienced by artiſts and collectors of prints and drawings, from the want of portfolios of dimenſions capable of incloſing large ſubjects, and as I alſo well know that the means uſed by the Society for the Encouragement of Arts, &c. to promulgate knowledge and uſeful information are earneſt as they are extenſive, I am induced to lay before that body a port-folio of my conſtruction, which I perſuade myſelf poſſeſſes every advantage that can be wiſhed.

The difficulty, or rather the impoſſibility of obtaining caſes or port-folios, as large as are ſometimes requiſite, has given riſe to many expenſive contrivances, to the ſame end; or large prints,

* "Quicquid ex phaenomenis non deducitur, *hypotheſis* vocanda eſt." Princ. LIII. in Bruckeri Hiſt. Crit. Phil. Tom. IV. p. 646.

† Society of Arts, 1803.

prints, &c. muſt be kept in rolls, to their almoſt certain deſtruction, by frequency of rolling; or at leaſt they are thus expoſed to the danger of being cruſhed by accident. Milled paſteboards, of which port-folios are made, are not manufactured above a certain moderate ſize: to exceed that ſize in a port-folio, is an undertaking of no inconſiderable trouble, in paſting, glueing, and preſſing them together. On inquiring of Mr. Newman, of Soho-ſquare, (a manufacturer of theſe articles) how he managed to make port-folios above the ordinary dimenſions, he informed me, it was an undertaking of trouble, and related his having made one for a gentleman, by attaching ſixteen of the largeſt milled boards together; that the materials alone coſt five guineas; and that its weight was greater than one man could lift.

Method of conſtructing large port-folios.

My method of conſtruction obviates all diſadvantages—weight, expence, and trouble; and port folios of any dimenſions may very readily be manufactured by the ſimple application of two ſtraining-frames, covered on both ſides with canvas, and papered; and connected, as all port-folios are, by leather at the back, or with wooden backs, the ſides being connected by hinges. Thus a port-folio may be made capable of holding the largeſt cartoons, maps, and prints; and poſſeſſing another great advantage, beſides that of not bellying or ſwagging, when laid againſt a wall, as thoſe conſtructed of paſteboard do, to their own deſtruction, and material injury of the things they contain.

A frame of four feet by three will be ſtrong enough, if made of deal. The ſtiles are four inches wide by half an inch thick; and they have a middle upright ſtile of the ſame width, with angle pieces at the corners, as is ſhown in the engraving, *Fig.* 1. A frame of much greater dimenſions may require two middle upright ſtiles; and, if very large, a middle longitudinal ſtile, as is ſhown in the engraving, *Fig.* 2.

If the frames are made of mahogany, they need not exceed 3-16ths of an inch in thickneſs; but, of whatever wood they are made, it muſt be well ſeaſoned, or they will warp. A padlock may be applied to ſuch caſes, for the protection of their contents.

On this conſtruction I have made two port-folios, one of which I have had in uſe theſe ten years. A handſome one, of tolerably large dimenſions, I have ſent with this paper, for

 the

Method of constructing large port-folios.

the inſpection of the Society. The outer ſtiles of it are of mahogany, which, beaded, forms the out edge in a neat manner. Its ſimple formation, its lightneſs, and its firm flatneſs, muſt be obvious to every one; and I am of opinion the Society will obtain the thanks of all collectors and artiſts (if they think it worthy of inſertion in the Volume of their Tranſactions) by making this ſimple matter publicly known. For my own part, I ſhall be highly gratified in having contributed to the comfort of artiſts and collectors, in preſerving their valuable reſearches.

I am, Sir,

Your obedient Servant,

JAMES MALTON.

Norton-ſtreet, June 25, 1802.

Reference to the Engraving, Plate VIII.

Fig. 4. One of the ſides of the frame for a port-folio, the dimenſions four feet by three: it may be made of deal or fir wood. The ſtiles, four inches wide by half an inch thick; the middle upright ſtile to be of the ſame width. It ſhould have angle pieces within the corners, to keep them firm.

Fig. 3. Shows a ſide of another frame, where much larger dimenſions are required; it ſhould then have two upright ſtiles, and a middle longitudinal ſtile, all within the frame, and angle pieces at the corners. If it is made of mahogany, inſtead of deal, the ſtiles may be reduced nearly one-fourth in breadth and thickneſs; the wood, in either caſe, ſhould be well ſeaſoned, that it may not be liable to warp.

XVII.

Experiments and Calculations relative to Phyſical Optics, By THOMAS YOUNG, *M. D. F. R. S. From the Philoſophical Tranſactions for* 1804.

(Concluded from p. 64.)

Exp. 2. So likewiſe the creſted fringes of Grimaldi.

Exper. 2. THE creſted fringes deſcribed by the ingenious and accurate GRIMALDI, afford an elegant variation of the preceding experiment, and an intereſting example of a calculation grounded on it. When a ſhadow is formed by an object which has a rectangular termination, beſides the uſual external

ternal fringes, there are two or three alternations of colours, beginning from the line which biſects the angle, diſpoſed on each ſide of it, in curves, which are convex towards the biſecting line, and which converge in ſome degree towards it, as they become more remote from the angular point. Theſe fringes are alſo the joint effect of the light which is inflected directly towards the ſhadow, from each of the two outlines of the object. For, if a ſcreen be placed within a few inches of the object, ſo as to receive only one of the edges of the ſhadow, the whole of the fringes diſappear. If, on the contrary, the rectangular point of the ſcreen be oppoſed to the point of the ſhadow, ſo as barely to receive the angle of the ſhadow on its extremity, the fringes will remain undiſturbed.

II. COMPARISON OF MEASURES, DEDUCED FROM VARIOUS EXPERIMENTS.

If we now proceed to examine the dimenſions of the fringes, under different circumſtances, we may calculate the differences of the lengths of the paths deſcribed by the portions of light, which have thus been proved to be concerned in producing thoſe fringes; and we ſhall find, that where the lengths are equal, the light always remains white; but that, where either the brighteſt light, or the light of any given colour, diſappears and reappears, a firſt, a ſecond, or a third time, the differences of the lengths of the paths of the two portions are in arithmetical progreſſion, as nearly as we can expect experiments of this kind to agree with each other. I ſhall compare, in this point of view, the meaſures deduced from ſeveral experiments of Newton, and from ſome of my own.

Compariſon of meaſures.

In the eighth and ninth obſervations of the third book of Newton's Optics, ſome experiments are related, which, together with the third obſervation, will furniſh us with the data neceſſary for the calculation. Two knives were placed, with their edges meeting at a very acute angle, in a beam of the ſun's light, admitted through a ſmall aperture; and the point of concourſe of the two firſt dark lines bordering the ſhadows of the reſpective knives, was obſerved at various diſtances. The reſults of ſix obſervations are expreſſed in the firſt three lines of the firſt Table. On the ſuppoſition that the dark line is produced by the firſt interference of the light reflected from the edges of the knives, with the light paſſing in a ſtraight line between them,

Compariſon of meaſures.

we may aſſign, by calculating the difference of the two paths, the interval for the firſt diſappearance of the brighteſt light, as it is expreſſed in the fourth line. The ſecond Table contains the reſults of a ſimilar calculation, from Newton's obſervations on the ſhadow of a hair; and the third, from ſome experiments of my own, of the ſame nature: the ſecond bright line being ſuppoſed to correſpond to a double interval, the ſecond dark line to a triple interval, and the ſucceeding lines to depend on a continuation of the progreſſion. The unit of all the Tables is an inch.

Table I. *Obſ.* 9. N.

Diſtance of the knives from the aperture - - -						101.
Diſtances of the paper from the knives	$1\frac{1}{2}$,	$3\frac{1}{3}$,	$8\frac{3}{5}$,	32,	96,	131.
Diſtances between the edges of the knives, oppoſite to the point of concourſe -	.012,	.020,	.034,	.057,	.081,	087.
Interval of diſappearance	.0000122,	.0000155,	.0000182,	.0000167,	.0000166,	.0000166.

Table II. *Obſ.* 3. N.

Breadth of the hair - - - - -		$\frac{1}{280}$.
Diſtance of the hair from the aperture - - -		144.
Diſtances of the ſcale from the aperture - -	150,	252.
(Breadths of the ſhadow - - -	$\frac{1}{54}$,	$\frac{1}{9}$.)
Breadth between the ſecond pair of bright lines	$\frac{2}{47}$,	$\frac{4}{17}$.
Interval of diſappearance, or half the difference of the paths - - - -	.0000151,	.0000173.
Breadth between the third pair of bright lines	$\frac{4}{73}$	$\frac{3}{10}$.
Interval of diſappearance, $\frac{1}{4}$ of the difference	.0000130,	.0000143.

Table III. *Exper.* 3.

Breadth of the object . - - - -	.434.
Diſtance of the object from the aperture - - -	125.
Diſtance of the wall from the aperture - - -	250.
Diſtance of the ſecond pair of dark lines from each other	1.167.
Interval of diſappearance, $\frac{1}{3}$ of the difference - -	.0000149.

Exper.

Exper. 4.

Breadth of the wire					.083.
Diſtance of the wire from the aperture					32.
Diſtance of the wall from the aperture					250.
(Breadth of the ſhadow by three meaſurements	.815,	.826,	or	.827; mean,	.823.)
Diſtance of the firſt pair of dark lines	1.165,	1.170,	or	1.160; mean,	1.165.
Interval of diſappearance					.0000194.
Diſtance of the ſecond pair of dark lines	1.402,	1.395,	or	1.400; mean,	1.399.
Interval of diſappearance					.0000137.
Diſtance of the third pair of dark lines	1.594,	1.580,	or	1.585; mean,	1.586.
Interval of diſappearance					.0000128.

It appears, from five of the ſix obſervations of the firſt Table, in which the diſtance of the ſhadow was varied from about 3 inches to 11 feet, and the breadth of the fringes was increaſed in the ratio of 7 to 1, that the difference of the routes conſtituting the interval of diſappearance, varied but one-eleventh at moſt; and that, in three out of the five, it agreed with the mean, either exactly, or within $\frac{1}{160}$ part. Hence we are warranted in inferring, that the interval appropriate to the extinction of the brighteſt light, is either accurately or very nearly conſtant.

But it may be inferred, from a compariſon of all the other obſervations, that when the obliquity of the reflection is very great, ſome circumſtance takes place, which cauſes the interval thus calculated to be ſomewhat greater: thus, in the eleventh line of the third Table, it comes out one-ſixth greater than the mean of the five already mentioned. On the other hand, the mean of two of Newton's experiments and one of mine, is a reſult about one-fourth leſs than the former. With reſpect to the nature of this circumſtance, I cannot at preſent form a decided opinion; but I conjecture that it is a deviation of ſome of the light concerned, from the rectilinear direction aſſigned to it, ariſing either from its natural diffraction, by which the magnitude of the ſhadow is alſo enlarged, or from ſome other unknown cauſe. If we imagined the ſhadow of the wire, and the

Comparifon of meafures.

fringes neareft it, to be fo contracted that the motion of the light bounding the fhadow might be rectilinear, we fhould thus make a fufficient compenfation for this deviation; but it is difficult to point out what precife tract of the light would caufe it to require this correction.

The mean of the three experiments which appear to have been leaft affected by this unknown deviation, gives .0000127 for the interval appropriate to the difappearance of the brighteft light; and it may be inferred, that if they had been wholly exempted from its effects, the meafure would have been fomewhat fmaller. Now the analogous interval, deduced from the experiments of Newton on thin plates, is .0000112, which is about one-eighth lefs than the former refult; and this appears to be a coincidence fully fufficient to authorife us to attribute thefe two claffes of phenomena to the fame caufe. It is very eafily fhown, with refpect to the colours of thin plates, that each kind of light difappears and reappears, where the differences of the routes of two of its portions are in arithmetical progreffion; and we have feen, that the fame law may be in general inferred from the phenomena of diffracted light, even independently of the analogy.

The diftribution of the colours is alfo fo fimilar in both cafes, as to point immediately to a fimilarity in the caufes. In the thirteenth obfervation of the fecond part of the firft book, Newton relates, that the interval of the glaffes where the rings appeared in red light, was to the interval where they appeared in violet light, as 14 to 9; and, in the eleventh obfervation of the third book, that the diftances between the fringes, under the fame circumftances, were the 22d and 27th of an inch. Hence, deducting the breadth of the hair, and taking the fquares, in order to find the relation of the difference of the routes, we have the proportion of 14 to 9¼, which fcarcely differs from the proportion obferved in the colours of the thin plate.

We may readily determine, from this general principle, the form of the crefted fringes of Grimaldi, already defcribed; for it will appear that, under the circumftances of the experiment related, the points in which the differences of the lengths of the paths defcribed by the two portions of light are equal to a conftant quantity, and in which, therefore, the fame kinds of light ought to appear or difappear, are always found in equilateral hyperbolas, of which the axes coincide with the outlines of the fhadow,

ſhadow, and the aſymptotes nearly with the diagonal line. Such, therefore, muſt be the direction of the fringes; and this concluſion agrees perfectly with the obſervation. But it muſt be remarked, that the parts near the outlines of the ſhadow, are ſo much ſhaded off, as to render the character of the curve ſomewhat leſs decidedly marked where it approaches to its axis. Theſe fringes have a ſlight reſemblance to the hyperbolic fringes obſerved by Newton; but the analogy is only diſtant.

III. APPLICATION TO THE SUPERNUMERARY RAINBOWS.

Application of the doctrine of the interference of light to the ſupernumerary rainbows of Langwith and Daval.

The repetitions of colours ſometimes obſerved within the common rainbow, and deſcribed in the Philoſophical Tranſactions, by Dr. Langwith and Mr Daval, admit alſo a very eaſy and complete explanation from the ſame principles. Dr. Pemberton has attempted to point out an analogy between theſe colours and thoſe of thin plates; but the irregular reflection from the poſterior ſurface of the drop, to which alone he attributes the appearance, muſt be far too weak to produce any viſible effects. In order to underſtand the phenomenon, we have only to attend to the two portions of light which are exhibited in the common diagrams explanatory of the rainbow, regularly reflected from the poſterior ſurface of the drop, and croſſing each other in various directions, till, at the angle of the greateſt deviation, they coincide with each other, ſo as to produce, by the greater intenſity of this redoubled light, the common rainbow of 41 degrees. Other parts of theſe two portions will quit the drop in directions parallel to each other; and theſe would exhibit a continued diffuſion of fainter light, for 25° within the bright termination which forms the rainbow, but for the general law of interference, which, as in other ſimilar caſes, divides the light into concentric rings; the magnitude of theſe rings depending on that of the drop, according to the difference of time occupied in the paſſage of the two portions, which thus proceed in parallel directions to the ſpectator's eye, after having been differently refracted and reflected within the drop. This difference varies at firſt, nearly as the ſquare of the angular diſtance from the primitive rainbow: and, if the firſt additional red be at the diſtance of 2° from the red of the rainbow, ſo as to interfere a little with the primitive violet, the fourth additional red will be at a diſtance of nearly 2° more;

Application of the doctrine of the interference of light to the fupernumerary rainbows of Langwith and Daval.

more; and the intermediate colours will occupy a fpace nearly equal to the original rainbow. In order to produce this effect, the drops muft be about $\frac{1}{76}$ of an inch, or .013, in diameter: it would be fufficient if they were between $\frac{1}{70}$ and $\frac{1}{80}$. The reafon that fuch fupernumerary colours are not often feen, muft be, that it does not often happen that drops fo nearly equal are found together: but, that this may fometimes happen, is not in itfelf at all improbable: we meafure even medicines by dropping them from a phial, and it may eafily be conceived that the drops formed by natural operations may fometimes be as uniform as any that can be produced by art. How accurately this theory coincides with the obfervation, may beft be determined from Dr. Langwith's own words.

"August the 21ft, 1722, about half an hour paft five in the evening, weather temperate, wind at north-eaft, the appearrance was as follows: The colours of the primary rainbow were as ufual, only the purple very much inclining to red, and well defined: under this was an arch of green, the upper part of which inclined to a bright yellow, the lower to a more dufky green: under this were alternately two arches of reddifh purple, and two of green: under all, a faint appearance of another arch of purple, which vanifhed and returned feveral times fo quick, that we could not readily fix our eyes upon it. Thus the order of the colours was, I. Red, orange-colour, yellow, green, light blue, deep blue, purple. II. Light green, dark green, purple. III. Green, purple. IV. Green, faint vanifhing purple. You fee we had here four orders of colours, and perhaps the beginning of a fifth; for I make no queftion but that what I call the purple, is a mixture of the purple of each of the upper feries with the red of the next below it, and the green a mixture of the intermediate colours. I fend you not this account barely upon the credit of my own eyes; for there was a clergyman and four other gentlemen in company, whom I defired to view the colours attentively, who all agreed, that they appeared in the manner that I have now defcribed. There are two things which well deferve to be taken notice of, as they may perhaps direct us, in fome meafure, to the folution of this curious phenomenon. The firft is, that the breadth of the firft feries fo far exceeded that of any of the reft, that, as near as I could judge, it was equal to them all taken together. The fecond is, that I have never obferved thefe inner orders of

of colours in the lower parts of the rainbow, though they have often been incomparably more vivid than the upper parts, under which the colours have appeared. I have taken notice of this so very often, that I can hardly look upon it to be accidental; and, if it should prove true in general, it will bring the disquisition into a narrow compass; for it will show that this effect depends upon some property which the drops retain, whilst they are in the upper part of the air, but lose as they come lower, and are more mixed with one another." Phil. Trans. Vol. XXXII. p. 243.

From a consideration of the nature of the secondary rainbow, of 54°, it may be inferred, that if any such supernumerary colours, were seen attending this rainbow, they would necessarily be external to it, instead of internal. The circles sometimes seen encompassing the observer's shadow in a mist, are perhaps more nearly related to the common colours of thin plates as seen by reflection.

IV. ARGUMENTATIVE INFERENCE RESPECTING THE NATURE OF LIGHT.

Argumentative inferences.

The experiment of Grimaldi, on the crested fringes within the shadow, together with several others of his observations, equally important, has been left unnoticed by Newton. Those who are attached to the Newtonian theory of light, or to the hypotheses of modern opticians, founded on views still less enlarged, would do well to endeavour to imagine any thing like an explanation of these experiments, derived from their own doctrines; and, if they fail in the attempt, to refrain at least from idle declamation against a system which is founded on the accuracy of its application to all these facts, and to a thousand others of a similar nature.

Homogeneous light has opposite qualities at equal distances along its course, by which neutralization or extinction of the light may be effected.

From the experiments and calculations which have been premised, we may be allowed to infer, that homogeneous light, at certain equal distances in the direction of is motion, is possessed of opposite qualities, capable of neutralising or destroying each other, and of extinguishing the light, where they happen to be united; that these qualities succeed each other alternately in successive concentric superficies, at distances which are constant for the same light, passing through the same medium. From the agreement of the measures, and from the similarity of the phenomena, we may conclude, that

that these intervals are the same as are concerned in the production of the colours of thin plates; but these are shown, by the experiments of Newton, to be the smaller, the denser the medium; and, since it may be presumed that their number must necessarily remain unaltered in a given quantity of light, it follows of course, that light moves more slowly in a denser, than in a rarer medium: and this being granted, it must be allowed, that refraction is not the effect of an attractive force directed to a denser medium. The advocates for the projectile hypothesis of light, must consider which link in this chain of reasoning they may judge to be the most feeble; for, hitherto, I have advanced in this Paper no general hypothesis whatever. But, since we know that sound diverges in concentric superficies, and that musical sounds consist of opposite qualities, capable of neutralising each other, and succeeding at certain equal intervals, which are different according to the difference of the note, we are fully authorised to conclude, that there must be some strong resemblance between the nature of sound and that of light.

Light moves more slowly in denser mediums.

Light and sound strongly resemble each other.

There is probably no inflecting medium.

I have not, in the course of these investigations, found any reason to suppose the presence of such an inflecting medium in the neighbourhood of dense substances as I was formerly inclined to attribute to them; and, upon considering the phenomena of the aberration of the stars, I am disposed to believe, that the luminiferous ether pervades the substance of all material bodies with little or no resistance, as freely perhaps as the wind passes through a grove of trees.

Practical application of the facts.

The observations on the effects of diffraction and interference, may perhaps sometimes be applied to a practical purpose, in making us cautious in our conclusions respecting the appearances of minute bodies viewed in a microscope. The shadow of a fibre, however opaque, placed in a pencil of light admitted through a small aperture, is always somewhat less dark in the middle of its breadth than in the parts on each side. A similar effect may also take place, in some degree, with respect to the image on the retina, and impress the sense with an idea of a transparency which has no real existence: and, if a small portion of light be really transmitted through the substance, this may again be destroyed by its interference with the diffracted light, and produce an appearance of partial opacity, instead of uniform semitrans-

parency. Thus, a central dark ſpot, and a light ſpot ſurrounded by a darker circle, may reſpectively be produced in the images of a ſemitranſparent and an opaque corpuſcle; and impreſs us with an idea of a complication of ſtructure which does not exiſt. In order to detect the fallacy, we may make two or three fibres croſs each other, and view a number of globules contiguous to each other; or we may obtain a ſtill more effectual remedy by changing the magnifying power; and then, if the appearance remain conſtant in kind and in degree, we may be aſſured that it truly repreſents the nature of the ſubſtance to be examined. It is natural to inquire whether or no the figures of the globules of blood, delineated by Mr. Hewſon in the Phil. Tranſ. Vol. LXIII. for 1773, might not in ſome meaſure have been influenced by a deception of this kind: but, as far as I have hitherto been able to examine the globules, with a lens of one-fiftieth of an inch focus, I have found them nearly ſuch as Mr. Hewſon has deſcribed them.

REMARKS ON THE COLOURS OF NATURAL BODIES.

Exper. 5. I have already adduced, in illuſtration of Newton's compariſon of the colours of natural bodies with thoſe of thin plates, Dr. Wollaſton's obſervations on the blue light of the lower part of a candle, which appears, when viewed through a priſm, to be divided into five portions. I have lately obſerved a ſimilar inſtance, ſtill more ſtrongly marked, in the light tranſmitted by the blue glaſs ſold by the opticians. This light is ſeparated by the priſm into ſeven diſtinct portions, nearly equal in magnitude, but ſomewhat broader, and leſs accurately defined, towards the violet end of the ſpectrum. The firſt two are red, the third is yellowiſh green, the fourth green, the fifth blue, the ſixth bluiſh violet, and the ſeventh violet. This diviſion agrees very nearly with that of the light reflected by a plate of air $\frac{1}{6840}$ of an inch in thickneſs, correſponding to the 11th ſeries of red and the 18th of violet. A ſimilar plate of metallic oxide, would perhaps be about $\frac{1}{15000}$ of an inch in thickneſs. But it muſt be confeſſed, that there are ſtrong reaſons for believing the colouring particles of natural bodies in general to be incomparably ſmaller than this; and it is probable that the analogy, ſuggeſted by Newton, is ſomewhat leſs cloſe than he

On the colours of natural bodies.

he imagined. The light reflected by a plate of air, at any thicknefs nearly correfponding to the 11th red, appears to the eye to be very nearly white; but, under favourable circumftances, the 11th red and the neighbouring colours may ftill be diftinguifhed. The light of fome kinds of coloured glafs is pure red; that of others, red with a little green: fome intercept all the light, except the extreme red and the blue. In the blue light of a candle, expanded by the prifm, the portions of each colour appear to be narrower, and the intervening dark fpaces wider, than in the analogous fpectrum derived from the light reflected from a thin plate. The light of burning alcohol appears to be green and violet only. The pink dye fold in the fhops, which is a preparation of the carthamus, affords a good fpecimen of a yellow green light regularly reflected, and a crimfon probably produced by tranfmiffion.

VI. EXPERIMENT ON THE DARK RAYS OF RITTER.

Dark rays of Ritter obferved alfo by Dr. Wollafton.

Exper. 6. The exiftence of folar rays accompanying light, more refrangible than the violet rays, and cognifable by their chemical effects, was firft afcertained by Mr. Ritter: but Dr. Wollafton made the fame experiments a very fhort time afterwards, without having been informed of what had been done on the Continent. Thefe rays appear to extend beyond the violet rays of the prifmatic fpectrum, through a fpace nearly equal to that which is occupied by the violet. In order to complete the comparifon of their properties with thofe of vifible light, I was defirous of examining the effect of their reflection from a thin plate of air, capable of producing the well known rings of colours. For this purpofe, I formed an image of the rings, by means of the folar microfcope, with the apparatus which I have defcribed in the Journals of the Royal Inftitution, and I threw this image on paper dipped in a folution of nitrate of filver, placed at the diftance of about nine inches from the microfcope. In the courfe of an hour, portions of three dark rings were very diftinctly vifible, much fmaller than the brighteft rings of the coloured image, and coinciding very nearly, in their dimenfions, with the rings of violet light that appeared upon the interpofition of violet glafs. I thought the dark rings were a little fmaller than the violet rings, but the difference was not fufficiently great to be accurately afcertained; it might be as much as $\frac{1}{30}$ or $\frac{1}{40}$ of the diameters, but not greater. It

They are reflected from a thin plate of air with the fame modification of rings as vifible light is.

is

is the less surprising that the difference should be so small, as the dimensions of the coloured rings do not by any means vary at the violet end of the spectrum, so rapidly as at the red end. For performing this experiment with very great accuracy, a heliostate would be necessary, since the motion of the sun causes a slight change in the place of the image; and leather, impregnated with the muriate of silver, would indicate the effect with greater delicacy. The experiment, however, in its present state, is sufficient to complete the analogy of the invisible with the visible rays, and to show that they are equally liable to the general law which is the principal subject of this Paper. If we had thermometers sufficiently delicate, it is probable that we might acquire, by similar means, information still more interesting, with respect to the rays of invisible heat discovered by Dr. Herschel; but at present there is great reason to doubt of the practicability of such an experiment.

SCIENTIFIC NEWS, ACCOUNT OF BOOKS, &c.

National Institute.

THE class of mathematical and physical sciences of the French National Institute had a public sitting on the 6th Messidor (June 25.) The subjects appointed for prizes were the following: National Institute prizes.

Mathematics. "It is required to give a theory of the perturbations of the planet Pallas, discovered by M. Olbers." Planet Pallas.

The prize will be a gold medal, weighing one kilogramme, (35$\frac{1}{7}$oz. avoirdupois.)

Natural Philosophy. The class had proposed for the subject of a prize the following question. "To determine by experiment the different sources of carbon in vegetables." The concurrence is prorogued until the 1st Germinal in the year 13 (March 22, 1805.) Carbon in vegetables.

The concurrence for the prize upon the following question is also prolonged to the same period. "To determine by anatomical and chemical observations and experiments, what are the phenomena of the torpid state to which certain animals, such as marmots, dormice, &c. are subject during the winter, with respect to the circulation of the blood, respiration, and irritability; to investigate the causes of this sleep, and why it is peculiar to these animals." Torpid state of dormice, &c.

The

The value of thefe two prizes is doubled, and confifts in two kilogrammes of gold about 6800 francs each (£289.)

The clafs had propofed for the fecond time, the 15th Germinal of the year 10, as the fubject of a prize to be decreed in the public fitting of Meffidor, the following queftion.

Ferments.

"What are the characters which diftinguifh, in vegetable and animal matters, thofe bodies which ferve as ferments from thofe in which they produce the ftate of fermentation."

The memoirs received not having anfwered the conditions of the program, and the clafs confidering that this queftion has been four years before the public, the fubject is now withdrawn.

Aftronomy. Prize gained by Piazzi.

Aftronomy. Mr. Piazzi of Palermo has obtained the prize founded by M. De Lalande, in favour of the perfon who fhall have made the moft interefting obfervation, or written the moft ufeful memoir to the progrefs of aftronomy. Mr. Piazzi is the difcoverer of the planet Ceres.

Readings.

The readings delivered during the fitting were as follow:

1. Notice of the labours of the clafs during the year; the mathematical part by M. Delambre. 2. Notice of their labours; the phyfical part by M. Cuvier. 3. Account of a phyfico-mathematical theory of currents of water, by M. Prony. 4. A note refpecting the amelioration of fheep in the fouth of France, by M. Teffier. 5. General reflections on the productions of the vegetable kingdom in the Pyrenean mountains by M. Raymond. 6. Reflections on heat, by Count Rumford. 7. Extract of a memoir tending to illuftrate the œconomical hiftory of vegetables, cultivated or naturally growing in the Canaries, by M. Brouffonet.

Extract of a Letter from PROFESSOR BODE, *Aftronomer Royal, to Mr. A. F. Thoelden.*

Dated Berlin, Sept. 18.

Place of new planets.

ON the firft of September Mr. Harding difcovered, at Lilienthal, a new moveable ftar; it appears to be of the eighth magnitude; its motion retrograde towards the South. Probably this may be another new planet in the orbit of Ceres and Pallas.

1ft Sept.

1ſt Sept.	10h.—Even.	RA.	2°	21′		Decl.	0°	37′—N.		
5 —	11 8′	-	1	51	51″	-	0	11	26″	S.
8 —	8 11	-	1	29	28	-	0	47	19	-
10 —	8 15	-	1	12	55	-	1	11	55	-

The two laſt obſervations were made by Dr. Olbers, at Bremen. I received this intelligence yeſterday, but at preſent this ſmall planet (there are now three) is not diſcoverable, on account of the moon-ſhine.

Extract of a Letter from Mr. G. B. GRENOUGH.

* * * * There is an overſight in the haſty account I ſent you of the Abbé Melograni's blow-pipe. No proviſion is made for admitting the external air. This may be done by ſeveral means. It may be admitted through that part of the axis which is oppoſite the nozel; but as this would require ſome contrivance which would render it defective in ſimplicity, I ſhould prefer the addition of a ſmall valve opening inwards at that part of each veſſel which is immediately oppoſite the neck that joins them. Whether this method was uſed in the actual inſtrument I cannot, at this diſtance of time, recollect.

Correction of a detail in the account of Melograni's blow-pipe.

Organic Remains of a former World. An Examination of the Mineralized Remains of the Vegetables and Animals of the Antediluvian World, generally termed extraneous Foſſils. By JAMES PARKINSON, *Hoxton. Vol.* I. *containing the Vegetable Kingdom. 4to. pp.* 471. *with* 10 *Plates* (£2 2 0) *Robſon, &c.*

AS it would be impracticable in the general announcement of a work of the magnitude and importance of the preſent to give the reader any well filled outline of the plan, I muſt confine myſelf to ſtate in few words, that the deſcription and elucidation of organic foſſil bodies, though in many points of view of the higheſt intereſt, has not yet conſtituted the ſubject of a general treatiſe; notwithſtanding the various conſiderations reſpecting the local, the individual, and the chemical hiſtory of theſe wonderful bodies have occupied the reſearches of many able philoſophers in detached memoirs; and that the compoſition of ſuch an hiſtory demands a great exertion of labour, in conſulting and digeſting what has been done

Parkinſon's organic remains.

Parkinſon's organic remains.

done by others, an accurate, geological, and chemical view of the facts upon which ſcientific concluſions or deductions may be grounded, and above all, an ardent curioſity, with adequate leiſure and means to become acquainted with the ſpecimens themſelves:—And, after this preface, I muſt add, that fortunately for the advancement of this branch of knowledge, the author of the preſent work has amply vindicated his claim to theſe requiſites.

The work is written in the form of letters, and enlivened by an adoption of the ſtyle and ſome of the incidents of a traveller. Much of uſeful ſcience and clear explanations of the ſubject are to be found in the pages of this volume. The abridgements from other authors are full, and the quotations always given with the volume and page, which I mention more readily, as a ſlovenly habit of looſe quotation, or not quoting at all, is but too common, even with our writers of merit. Nine plates of the ſubjects are given, which do the higheſt credit to the deſigner and engraver, Springſguth, for their delicacy and beauty, and conſiderable pains have been taken in colouring them. The frontiſpiece, repreſenting the Ark upon Mount Ararat, deſigned by Corbould, is happy and appropriate; though the artiſt has taken a pictorial licence to repreſent the ſun and the rainbow nearly on the ſame azimuth, for which neither opticians nor correct obſervers will be diſpoſed to excuſe him.

The volume yet to be publiſhed will contain the animal kingdom; a ſubject perhaps ſtill more attractive than the vegetable. In the remains of extinct vegetables, we appear to contemplate the phyſical economy of a world long ago obliterated; but in thoſe of animals, we ſeem to behold ſomething that gives a glimpſe into the moral relations of thoſe beings of paſt ages who exiſted upon its ſurface.

Apparatus for Filtration by Sir H. C. Englefield Bar.t &c.

Structure for purifying a Stream of Water.

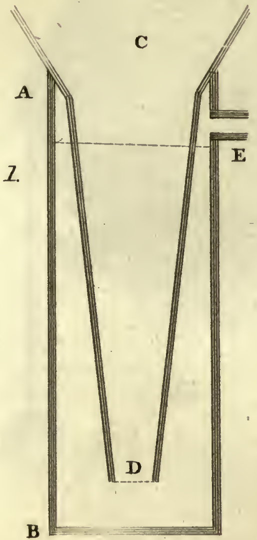


Fig. 1.

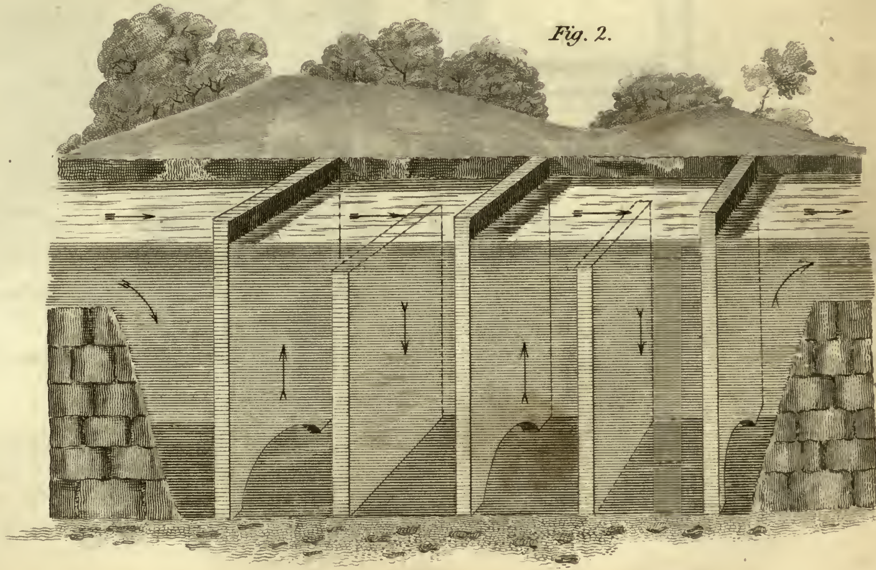
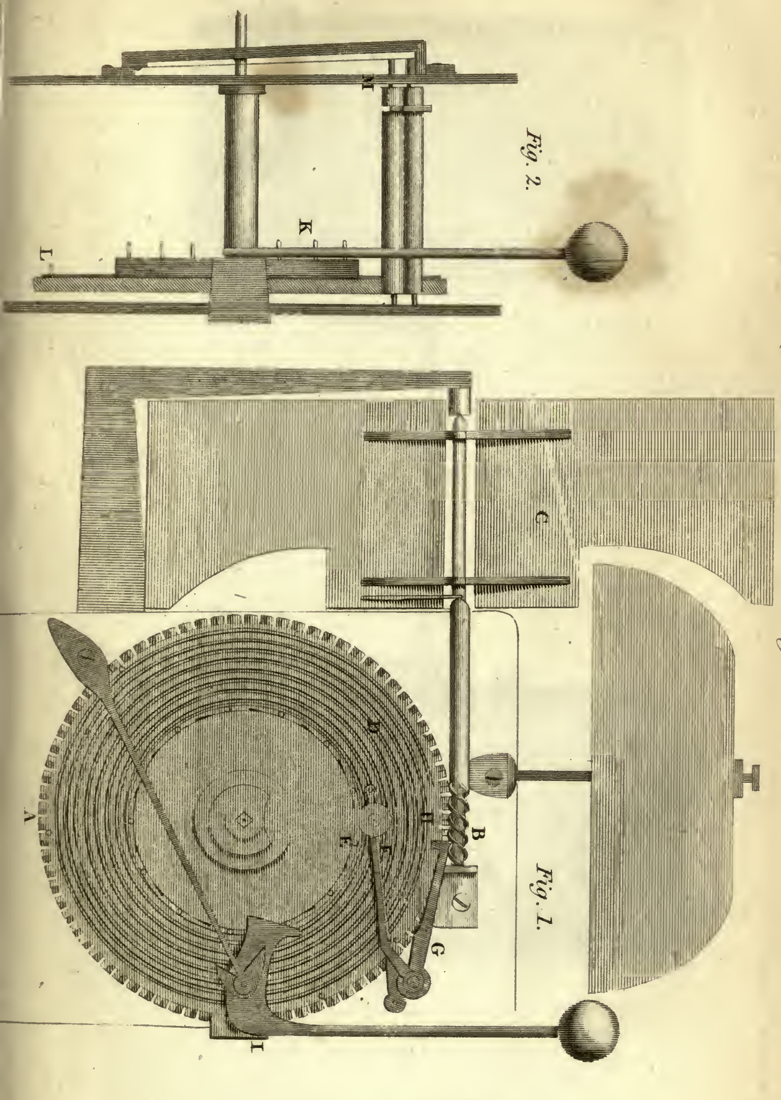


Fig. 2.

Drawn by Blunt.

Engraved by Mutlow.

Striking part of a Clock, by Mr. Prior.



Mutlow Sc. Russell Co.

Mr. Antiss Indicator of the Number of Draughts from a Mine.

Fig. 2.

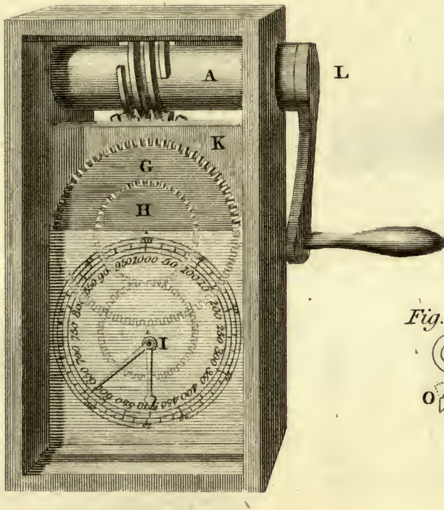


Fig. 3.

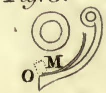
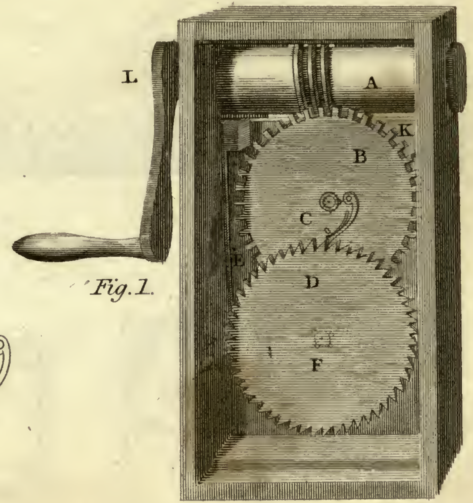


Fig. 1.



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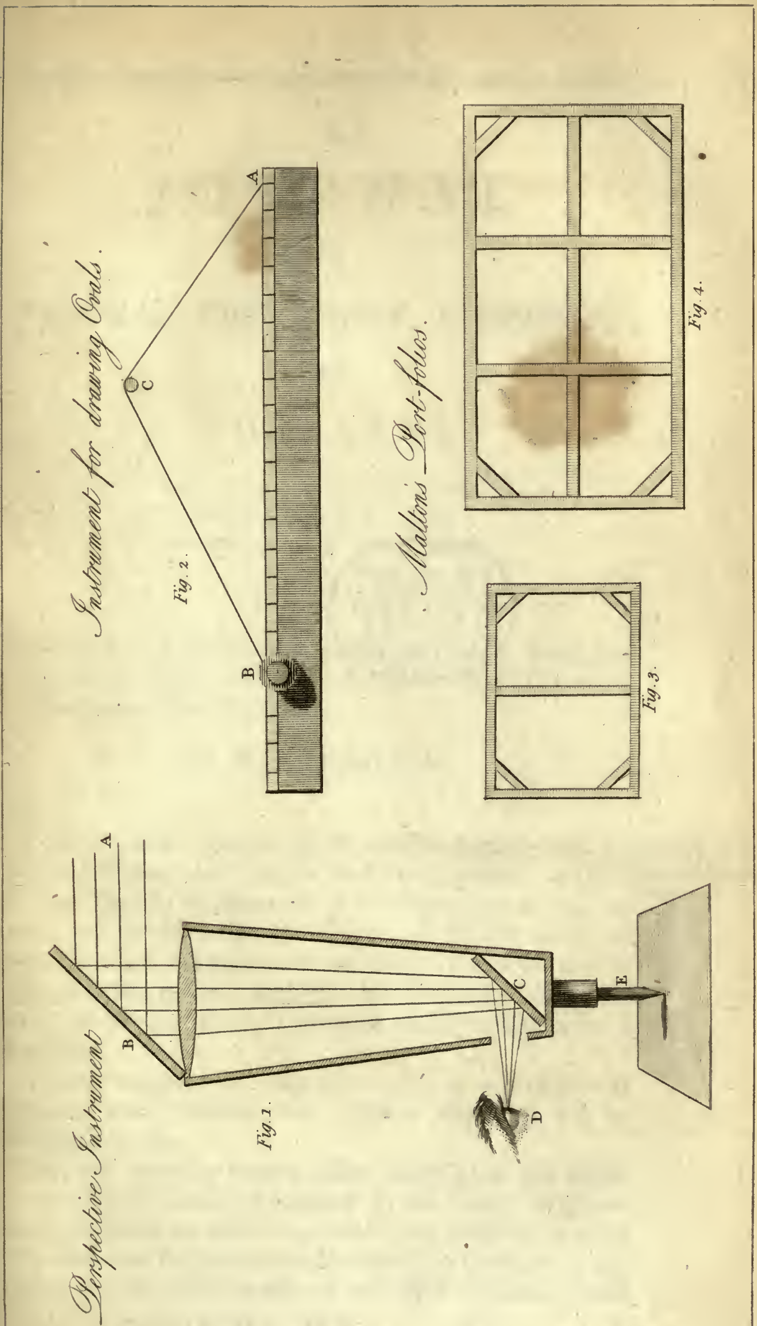


Perspective Instrument

Fig. 1.

Instrument for drawing Ovals.

Fig. 2.

Malton's Port-folios.

Fig. 3.

Fig. 4.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

NOVEMBER, 1804.

ARTICLE I.

Description of a Tallow Lamp, which regulates its Supply by a spontaneous Movement. By Mr. John Whitley Boswell. *Communicated by the Inventor.*

To Mr. NICHOLSON.

SIR,

THE constant attention which candles demand from the frequent snuffing they require, and the continued variation of their light in the intervals of this operation, on the one hand, and on the other the expence and trouble which oil lamps occasion, together with the disagreeable scent they in general produce, have rendered the contrivance of a good lamp for burning tallow, a desirable object to several for a long time. Description of a lamp for burning tallow.

I hope, therefore, the description I send of a lamp for this purpose, which possesses some valuable properties, will be acceptable to you.

The best lamp for burning tallow which I had seen before I contrived this, was one invented by Mr. March, of Barnstaple, in which the tallow kept continually melted in a vessel suspended over the flame, was admitted by a small pipe to the burner, and the supply regulated by a kind of cock attached to

Defcription of a lamp for burning tallow.

to the pipe: But this method occafioned a wafte from the evaporation of the melted tallow, and befides, required much attendance to regulate the cock and keep the tallow veffel at a proper diftance from the flame; for the alteration of the temperature of the melted tallow, which was very frequent from many caufes, continually changing its degree of fluidity, caufed it to vary in its fupply through the pipe proportionally.

It occurred to me fome time after I faw this lamp, that one might be contrived in which the tallow fhould only be melted as it was confumed, and other of the inconveniences be removed to which the firft was fubject; and I made then a rough drawing of a lamp for this purpofe, but other more interefting matters prevented my having one made on that plan before laft December: It confifted of a trough fupported in an inclination of about 45 degrees, fimilar to the one in the figure, which held a fquare prifm of tallow; under this was placed a little oblong veffel of tin, formed fo as to move forwards and backwards like a drawer, in front of which was placed a burner: a couple of wires placed acrofs the lower part of the trough, prevented the tallow from fliding down beyond them, as it otherwife would from the obliquity of its fupport; but as it melted by the heat from the burner, it fell in drops into the little drawer, fo as to fupply the flame, while the degree of this fupply was regulated by moving the drawer in or out, fo as to bring the burner nearer to, or further from the tallow. In this ftate, though it formed a very ufeful lamp, it was not fo perfect as I could wifh; every alteration of temperature of the apartment (which in cold weather happens frequently) required a proportional alteration in the diftance of the burner from the tallow, to prevent the little drawer being either overflowed by a fuperabundance of melted tallow, or the flame ceafing from a deficiency of fupply. The attendance this circumftance required induced me to contrive the prefent lamp, or rather to improve the former, in manner following.

I conceived that if the little pan, which held the melted tallow and the burner, was placed on the arm of a balance inclined from 30 to 45 degrees from the perpendicular, and its weight compenfated by a counterpoife at the extremity of the other arm, that when more than a certain quantity of tallow

low ran into it, it would fink lower, and, in doing fo, move the burner farther forward from the trough at the fame time, and *vice verfâ*, and thus regulate the fupply fpontaneoufly: But as it was neceffary that the pan (holding a fluid) fhould always maintain an horizontal pofition during its movement, I imagined this might be effected by placing another arm beneath the balance-arm, and fixing a perpendicular piece, fo jointed to the extremity of each as to move frefh and fteadily, which piece fhould fupport an horizontal ftand for the pan.

Defcription of a lamp for burning tallow.

I had a lamp made in this manner, and found it anfwer my intention perfectly. It required no attendance whatfoever, but regulated its fupply with precifion; and afforded, likewife, an agreeable fpectacle, having in its movements fomewhat the air of thofe of an animal, from their exact relation to an evident object, and adopting themfelves to all its changes.

The lamp thus made affumed the form in the figure, Pl. IX. where T reprefents the inclined trough for holding the tallow; P the pan for catching the tallow as it melted, and holding the burner; F the flame and burner; R the perpendicular piece fuftaining a fupport for the pan; B the balance moving freely on its center in the ftand S, and jointed to R as before defcribed; *c* the parallel arm ufed to keep R perpendicular, and by its means horizontal; *w* the counter weight, placed fo as to move on the extremity of B by a fcrew. The firft frofty night fhewed me the neceffity of the addition of another part to remedy an inconvenience I had not forefeen; the dropping tallow affuming the form of an ificle, joined the pan to the trough, and thus ftopped its movement: As it was troublefome to break this off as it occurred, I contrived the following means to prevent its formation: A little oblong fliding-piece, the fame breadth as the trough, was placed clofe underneath its lower part; the extremity of this flide was made angular, that the melted tallow might fall from a fingle point; a wire reprefented at L, moved it back or forwards as required: By this flide the drop may be made to fall as clofe to the ftream of burning air from the flame as is thought fit, and all congelation prevented. I afterwards added a fort of reflector to the lamp, which confiderably improved the light, not merely acting as its name implies, but alfo occafioning an

increafe

Defcription of a lamp for burning tallow.

increafe in the current of air which paffed the flame, rendered it fomewhat more bright, and confiderably more fteady. This part is reprefented at D in the figure, and confifts of two fide-pieces placed before the trough, but fo as not to obftruct the tallow, and bent forward to form an angle with its fides. To improve the light, I alfo ufe in the lamp five fmall diftinct wicks, of three threads each, inftead of one large wick, placed near each other in a row, which renders the flame clear and free from fmoke, and thereby alfo prevents any unpleafant fcent. I ufed this lamp for more than three months, and found it very convenient for reading or writing by, as when once lighted it required no farther attention, and kept its light at nearly the fame height at all times, and of the fame degree of intenfity; and I can further recommend it, as yielding the greateft light at the fmalleft coft, in proportion, of any invention yet made public which is applicable to domeftic ufes.

I have had two lamps of this kind carefully made by Mr. Lloyd, No. 178, Strand, and left with him as models: In one of which the chief object is cheapnefs, as far as confiftent with perfection and neatnefs; in the other, to form an elegant utenfil for the ftudy, office, or chamber; and any gentleman may be fupplied with them there, made exactly to my plan, on reafonable terms; and as Mr. Lloyd is the only perfon at prefent appointed to make them, and inftructed fully in the mechanifm, they can be had perfect from him only.

It is probable wax might alfo be burned in this lamp as well as tallow: of this I cannot fpeak from experience, but mention it, as I know fome would prefer to ufe it.

I am, Sir,

Your very humble fervant,

J. WHITLEY BOSWELL.

Oct. 1, 1804.

Account

II.

Account of a ſucceſsful Caſe in which Deafneſs was cured by Puncture of the Membrana Tympani. By M. Maunoir, of Geneva. Communicated by ALEXANDER MARCET, *M.D. Phyſician to Guy's Hoſpital.*

To Mr. NICHOLSON.

SIR,

THE incloſed communication on the effects produced in a caſe of deafneſs by the perforation of the tympanum of the ear, is extracted from a letter which I have juſt received from Mr. Maunoir, an eminent ſurgeon at Geneva. The hiſtory of this caſe, drawn up by ſuch an intelligent and accurate obſerver, appears to me curious, both from the phyſiological remarks which it contains, and from the ſtriking illuſtration which it affords of a new mode of practice lately propoſed in this country. And, as this ſubject ſeems fully as intereſting in its philoſophical point of view, as in its connection with practical ſurgery, I ſhould wiſh, if you think it proper, that you would give it a place in the next number of your valuable Journal. I have the honour to be, Sir, Introductory letter.

Your obedient humble ſervant,

ALEXANDER MARCET.

St. Mary Axe, Oct. 16, 1804.

Extract of a Letter from Mr. Maunoir, ſurgeon at Geneva, dated 5th September, 1804.

"Mr. F. of Geneva, aged forty years, had loſt his hearing, ſeveral years ago, in conſequence of a long affection of the poſterior part of the internal fauces, and it was evident, that, in this caſe, the euſtachian tubes were obliterated. In condenſing the air in his mouth and noſtrils, he was totally unable to diſtend the membrane of the tympanum, a thing which is very eaſy to do for any one who enjoys a perfect ſenſe of hearing. He could ſcarcely hear, even when you ſhouted in his ears, and although ſo long deaf, he had not acquired the faculty of underſtanding by the motion of the lips. After peruſing the paper which I lately publiſhed in the Bibliothéque Britannique, Mr. F. conceived that he might be one of thoſe in whom the hearing could be recovered by the perforation of the A gentleman aged 40 years had been ſeveral years extremely deaf.

Puncture of the tympanum of one ear.

the tympanum. On the 14th of July laft, I performed that operation on his right ear, in prefence of Mr. Jurine, with a very fmall trocar, about one eighth of an inch in diameter. At the inftant that the inftrument was withdrawn, we fpoke to him in a low voice; but inftead of anfwering, he remained immoveable in his chair, and feemed ftupified.—Then he exclaimed " For God's fake gentlemen, don't cry out fo loud; you hurt me!" I then began to walk about the room.—The fqueaking of my boots made him fhudder and jump in his chair; and he covered his ear with his hand. The fnapping of the fingers quite diftracted him, and feemed to produce upon him an effect fimilar to that of a piftol fuddenly fired in the ear of a perfon not aware of it. When fpoken to, though in a whifper, he complained that we fpoke too loud. Yet in fpite of this exceffive fenfibility to very inconfiderable noifes, there were other founds, not lefs diftinct, which he did not hear, even at a very little diftance; fuch for inftance as the *ticking* of his watch. So that his hearing feemed to be fometimes too dull, fometimes too acute; becaufe, no doubt, the organ had loft the faculty of adjufting itfelf to the different modulations of found. A week after this, however, Mr. F. had loft that exceffive fenfibility which rendered acute founds almoft intolerable to him, and in fact he had already *again learnt* to hear. He then wifhed to have the other ear perforated in the fame manner. I performed this operation, but without effect.—Twenty days after this fecond operation, Mr. F. called upon me. I examined his ears in a fine fun fhine, and could fee in the right ear the membrane of the tympanum traverfed in its exterior part by a fmall cicatrix, having in its center a fmall hole which was hardly perceptible. Yet the hearing was fcarcely diminifhed fince the firft operation. Fearing that this fmall aperture might clofe itfelf entirely, Mr. F. defired that I would perforate again the membrane of that ear, an operation which was repeated without giving him any pain, but not without a flight increafe of fenfibility in his hearing. I afterwards examined the left ear, and having placed the meatus in the direction of the fun's rays. I diftinctly perceived a falfe membrane adhering to the whole circumference of the paffage, and perfectly fimilar, in its form and fituation, to that of the tympanum, the diftance between the two membranes being fcarcely one fixth part of an inch. I took off at once this falfe

Inftant reftoration of hearing with extreme fenfibility.

Some founds more audible than others.

The other ear punctured without effect.

Caufe of this failure.

falſe membrane with a pair of tweezers, and diſcovered behind it, the tympanum, quite ſound and entire. I thought it probable that, in the ſecond operation, I had only touched this falſe membrane. I immediately perforated the true one, and was ſurprized (as well as my brother who attended this operation), to find that, although Mr. F. had already learnt again to hear with the right ear, yet the reſtoration of his hearing in the left, occaſioned in him the ſame effects of wonder, and of exceſſive ſenſibility to the leaſt unexpected noiſe, which he had experienced after the firſt operation. This inconvenience, however, has not been of longer duration in this ear, than it had been in the other."

Repetition of the operation with effect and ſimilar phenomena.

III.

Letter from Mr. TIMOTHY SHELDRAKE, *expoſing the Errors of M. Tingry reſpecting Copal Varniſhes; with ſome farther Inſtructions concerning the ſame.*

(Received Oct. 16, 1804.)

To Mr. NICHOLSON.

SIR,

M. TINGRY, profeſſor of chemiſtry, &c. in the academy of Geneva, has publiſhed a treatiſe on the art of making and applying varniſhes; with new obſervations and experiments on copal, &c. &c. A tranſlation of that work has lately appeared here, and at p. 157 of that tranſlation I find the following paſſage.

Tingry in his art of making varniſhes—

" Mr. Timothy Sheldrake mentions camphor as a medium for diſſolving copal in eſſence* and alcohol. He gives alſo another proceſs, in which ammonia (volatile alcaline ſpirit of ſal ammoniac) in the proportion of an eighth part of the eſſence employed, is ſubſtituted in the room of camphor.

" *Exper.* I. Of the three proceſſes which he deſcribes, I repeated two: thoſe which regard the ſolution of copal in eſſence and in alcohol by a mixture of camphor. *That with eſſence did not ſucceed.* The author himſelf announces that he

repeated Sheldrake's proceſſes for diſſolving copal in eſſence and did not ſucceed;—

* Throughout the book this word is uſed to deſcribe what we call ſpirit of turpentine.

always

always failed, except when he obtained the effence from apothecaries hall. It appears that this effence had by chance all thofe effential qualities which we endeavour to give it by time, and ftill more fpeedily by the influence of light.

nor with alcohol.

"*Exp.* II. The fame experiment repeated with pure alcohol *was attended with too little fuccefs to make the refult be confidered as a varnifh.* The alcohol appeared milky, and the copal formed at the bottom of the veffel a mafs which did not feem to have decreafed in volume. Next morning the interpofed part of the copal, which altered the limpidity of the alcohol, was precipitated, and adhered to the fides of the glafs. The procefs I have defcribed for fpirituous tincture of amber would give more hopes of fuccefs without any intermediate fubftance."

He infers that the compounds muft be imperfect and not varnifhes.

"In regard to the means propofed by the medium of ammonia, the faline nature of that liquid, *if the procefs fucceeds,* will not admit of the product being placed in the clafs of varnifhes deftined for delicate painting. *It is a kind of faponaceous compound, the ufe of which is not to be recommended in fuch cafes.*"

Obfervations in reply.

The paffage above quoted undoubtedly relates to a paper which I communicated feveral years ago to the Society for the Encouragement of Arts, &c.

This paper was wholly tranfcribed * into fome periodical publications, and parts of it into others; it may therefore be neceffary to obferve that when reference is made to the original paper, it muft be underftood to mean as it was printed in the Society's Tranfactions, and not to any imperfect extracts that may have been made from it.

Mr. Tingry ought not to have oppofed *fpeculation* againft *facts.*

Mr. Tingry fhould have been fenfible of the danger in oppofing a conjecture or opinion *purely fpeculative*, to what is defcribed as a *fact that had been demonftrated by experiment.* Since it is more probable that the conjecture fhould be unfounded, or the opinion untrue, than that a man fhould forfeit his reputation with the public by relating that, as having been demonftrated by experiment, which the perfon who contradicts him fays cannot, in the nature of things be true, even if the *procefs fucceeds:* As it is the intention of your valuable Journal to promote the caufe of fcience, and as that caufe will be as effectually

* See our Journal, I. 250.

nally ferved by the detection of errors, as by the promulgation of new facts, I truft you will favour me by inferting the following obfervations on thofe paffages which I have quoted from Mr. Tingry's work; beginning with that in which he attempts to demolifh one part of difcovery (if it may be fo called) without ufing one experiment as the inftrument of deftruction.

"In regard to the means propofed by the medium of ammonia, the faline nature of that liquid, *if the procefs fucceeds,* will not *admit of the product being placed in the* CLASS OF VARNISHES, deftined for delicate painting; it is a kind of *faponaceous compound, the ufe of which is not to be recommended in fuch cafes.*"

Inftances of pictures varnifhed with the copal many years ago, and now in perfect and excellent condition.

In oppofition to all this I have but a fimple fact to produce: viz. It is many years fince I diffolved copal by the procefs alluded to; and actually varnifhed feveral pictures with the folution fo prepared: thofe pictures appeared to fhine, or bear out, as artifts call it, as well as if they had been covered with any other varnifh; the colour of the varnifh has not changed in the leaft, which moft other varnifhes would have done in the fame time; they have ftood in a fmoky houfe in London the whole time, have been repeatedly wafhed with warm water, and cleaned with fpirit of turpentine; and notwithftanding all this, they ftill preferve their gloffy appearance undiminifhed. If a folution which is proved by the experience of many years to poffefs thefe properties in an eminent degree, does not deferve to be admitted into the clafs of varnifhes, it is not eafy to tell to what fubftance Mr. Tingry will grant that diftinction.

The ammonia probably forms no part of the compound; but this is of no practical confequence.

Having faid thus much for the fact, we may venture to proceed to the theory: When ammonia and copal were mentioned, the combination of both thofe fubftances with the fpirit of turpentine appeared to Mr. Tingry to be fo much a matter of courfe, that he took it for granted, and drew all the conclufions from it without making a fingle experiment to prove the reality of the fact, had he done fo, even he, with the ftrong prejudices he evidently has upon this fubject, might have *doubted if the ammonia entered into the compofition at all;* to me it appears, after attending carefully to the phenomena of the folution, that it *does not*; but without determining pofitively on the fubject, I fhall defcribe thofe phenomena, and leave your readers to judge for themfelves.

The

Particular deſcription of the proceſs for copal in ſp. of turpentine by ammonia.

The copal muſt be groſsly powdered and put into a convenient glaſs veſſel; the ſpirit of turpentine and ammonia muſt be well ſhaken together, and the mixture poured upon the copal; the veſſel cloſed, and ſo much heat applied as ſhall, as ſpeedily as poſſible, make the ſpirit boil ſo ſlowly that the bubbles may be counted as they riſe to the ſurface; if this is ſkilfully performed, the ſolution is ſeen to take place in the following manner; the mixture has a milky appearance when firſt applied to the copal; the upper ſurface firſt becomes clear, the cloud gradually ſubſides, and attaches itſelf to the copal, which ſwells and becomes opaque; a portion of it in a diſſolved ſtate gradually exudes with the air bubbles, and diffuſes itſelf in the ſpirit, and this continues till it has taken up as much as can be ſuſpended in it; no more is then diſſolved: It is on the ſkilful management of theſe circumſtances that the ſucceſs of the proceſs depends; for, if leſs heat than the degree required be given, the ſolution ſtops; if more, the bits of copal inſtead of being opaque and adhering together at the bottom of the veſſel, reſume their tranſparency, and are agitated ſeparately by boiling of the ſpirit; and when either of theſe accidents happened, I could never afterwards diſſolve the ſame portion of copal either in the ſame or any freſh portion of ſpirits. If I might venture to explain theſe phenomena, I ſhould ſay, that the ammonia gradually quits the ſpirit of turpentine, and unites with the copal, and reduces it to ſuch a ſtate, that with the aſſiſtance of the peculiar degree of heat that I have deſcribed, the ſpirit of turpentine is enabled to diſſolve ſo much as will form a varniſh; but, if either more or leſs heat is applied, the copal cannot be diſſolved, and then the operation is abortive.

The ſucceſs depends on the management of the circumſtances.

Excellence of this varniſh, &c.

Whether this explanation be ſatisfactory or not is of ſmall conſequence, the exiſtence of pictures in my poſſeſſion, which were varniſhed with this ſolution many years ago, and ſtill remain more perfect than they would have been if covered with any other varniſh, is ſufficient to prove that all Mr. Tingry has ſaid on this ſubject has not the ſmalleſt foundation in truth.

Hint which produced the diſcovery.

I had examined all the books which I had acceſs to, without getting any information reſpecting the ſolution of copal for the purpoſe of making varniſh, till in Lewis's tranſlation of Neuman's works I found the following, Vol. II. p. 27. " Solutions of copal have been greatly eſteemed as varniſhes, and the method of making the ſolution kept a ſecret in particular hands: Junker

Junker informs us that it *readily ſucceeds, if ſpirit of ſal-ammoniac, mixed with a due proportion of oil of ſpike or oil of turpentine,* be uſed as the menſtruum." Upon this tint I proceeded till I had diſcovered the proceſs I have deſcribed; but, though the varniſh anſwered every purpoſe I could wiſh, the trouble of making it being great, and the failure of the proceſs ſo frequent that I determined to ſeek for ſome more convenient method.

Having found that copal, when powdered and rubbed in the mortar with a ſmall quantity of camphor, became ſoft, and in a few minutes, united in one coherent maſs, I conjectured that by means of camphor, copal might be diſſolved in alcohol, and upon trial found that it might be ſo diſſolved, and rendered fit for the purpoſes of varniſh; this I did, not by way of experiment, but in conſiderable quantities and at various times; ſo often, that I am authoriſed to ſay, that if the materials are good, and the proceſs I have deſcribed in the Tranſactions of the Society for Encouragement of Arts be followed, there can be no difficulty in making a colourleſs ſpirit varniſh from copal; and as a proof that this may be done, I ſend you a ſmall quantity that I made ſome time ago, and which will enable you to judge whether it deſerves to be "*placed in the claſs of varniſhes.*" **Invention of camphor as a better medium than ammonia.**

But as it is improper, for many reaſons, to varniſh pictures with any ſubſtance diſſolved in alcohol, I tried to diſſolve the copal in ſpirit of turpentine by the intervention of camphor, and ſucceeded; not ſo eaſily indeed as in alcohol, but always with the certainty of ſucceſs, when the precautions I have deſcribed were obſerved; this has been uniformly the caſe with myſelf for ſeveral years, as well as ſeveral artiſts who have made the trial; and it has now been proved by many years experience that copal diſſolved, by means of camphor, in ſpirit of turpentine, is as colourleſs as the reſin itſelf and perhaps as durable, ſince pictures that have been varniſhed many years with it have been preſerved without the leaſt change even in the colour of the varniſh, which would not have been the caſe had any other ſubſtance been uſed. **Sp. of turpentine preferable to alcohol.** **This varniſh is alſo excellent.**

As Mr. Tingry's *experiments* lead to very different concluſions, I am compelled to ſuppoſe, either that they were inaccurately made, or that the copal, camphor, alcohol, and* ſpirit

* Mr. Tingry himſelf, *as he ſays*, tried to diſſolve copal in ſpirit of turpentine, and the proceſs did not ſucceed: his notion that the ſpirit

ſpirit of turpentine in his country are very different from the ſubſtances which are ſold by the ſame names in this.

General remarks.

My intention in communicating thoſe papers to the Society was to aſſiſt thoſe who are engaged in the practice of the arts, by making public a method of making a varniſh that muſt be valuable to them: and I have been induced to trouble you with this, in hopes that, by printing it, you may prevent thoſe falſe notions, which might be formed upon Mr. Tingry's experiments from gaining credit; to which you will allow me to add ſuch hints and improvements as may prevent diſappointment to thoſe who undertake to prepare this varniſh for themſelves inſtead of depending upon thoſe who make it for ſale; this every artiſt ſhould do if he reſolves to have it perfect; for, though I do not mean to ſuppoſe that every maker of varniſh will ſubſtitute inferior articles, yet it is certain that where the price of one article is higher, becauſe it is really valuable, and difficult to make, many dealers will ſubſtitute ſomething *inferior* that produces more profit to himſelf, without conſidering whether it may not be injurious to thoſe who are to uſe it.

Narrative of an artiſt who did not ſucceed in the proceſs.

I had ſeveral times given ſome of my varniſh to an artiſt, who liked it ſo well that he requeſted I would inſtruct him to make it himſelf, inſtead of making frequent applications to me for it: I procured him the neceſſary apparatus, and gave him a written account of the proceſs, with ſuch verbal inſtructions as I could add to it, and left him to procure his own materials and proceed to work by himſelf; he did ſo, and did not ſucceed in diſſolving the copal; he tried a ſecond and third time with no more ſucceſs; he then brought his materials to me, requeſted that I would try if I could diſſolve them; which experiment would determine whether his materials were bad, or whether there was any defect in his manner of attempting the proceſs: this I reſolved to undertake; uſing the ſame ſpirit

ſpirit I uſed had *by chance* obtained thoſe powers for diſſolving copal which *he endeavaurs to give it by time*, &c. is perfectly ridiculous. You and your Engliſh readers will know that in ſaying I did not ſucceed when I uſed ſpirit of turpentine from the colour ſhops, but always did when I got the ſpirit from Apothecaries Hall, I meant that at the latter place I always obtained the beſt article of the kind, inſtead of the inferior articles which are ſold by the *ſame name* in other places, though totally unfit for this purpoſe.

and

and copal which he had thrice attempted in vain to diſſolve; and at the ſame time ſubjecting another portion of my own materials to the ſame proceſs in every particular: I ſucceeded in both inſtances: the only difference was that the ſolution which I produced from the materials which had been tried by my friend was not ſo viſcid as that which was obtained from the freſh materials.

As the proceſs which I have uſed for ſome time paſt is different from and better than that which I communicated to the Society, and has been publiſhed in their Tranſactions, I truſt you will excuſe me for deſcribing it here; it is ſuch an one as may be ſucceſsfully uſed by any perſon upon a common fire, without any danger, and which any man of ſcience, or who may chuſe to adopt in a large way, may eaſily modify ſo as to anſwer his purpoſe. Improved proceſs.

Provide a ſtrong veſſel made of tin or other metal, it ſhould be ſhaped like a wine bottle and capable of holding two quarts; it will be convenient to have a handle ſtrongly rivetted to the neck; the neck ſhould be long and have a cork fitted to the mouth, but a notch or ſmall hole ſhould be made in the cork, that, when the ſpirit is expanded by heat, a ſmall portion may force its way through the hole, and thus prevent the veſſel from burſting. Safe and eaſy method of making copal varniſh.

Diſſolve half an ounce of camphor in a quart of ſpirit of turpentine, and put it into the veſſel; take a piece of copal the ſize of a large walnut, reduce it to coarſe powder or very ſmall pieces; put them into the tin bottle, faſten the cork down with a wire, and ſet it as quick as poſſible upon a fire ſo briſk as to make the ſpirit boil almoſt immediately; then keep it boiling very gently for about an hour, when ſo much of the copal will be diſſolved as will make a very good varniſh; or, if the operation has been properly begun, but enough copal has not been diſſolved, it may be again put on the fire, and by boiling it ſlowly for a longer time, it may be at laſt brought to the conſiſtence deſired.

The rationale of the operation I believe will be nearly as follows: copal and camphor have ſo ſtrong an affinity for each other, that when ſeparately powdered and then rubbed together, the copal abſorbs the camphor, ſwells, ſoftens, and becomes one coherent maſs; nor when once united do they eaſily ſeparate Explanation of the proceſs;

ſeparate again *. When the camphor is diſſolved in the ſpirit, the ſmall bits of copal thrown into it and inſtantly made to boil, the motion of ebullition keeps the camphor ſuſpended, moves the copal about in the ſpirits, the ſurface of each bit of copal is diſſolved, and the whole of it ſoftened and enlarged, and by continual boiling may be diſſolved; but my friend did not proceed in this way; he reduced his copal to a very fine powder, thinking, perhaps, that would facilitate the ſolution; he then added the camphor, diſſolved in the ſpirit of turpentine, but fearful of giving too much heat, ſet it by the ſide of his fire till it was warmed, then moved it into a warmer place, and at laſt made it boil; the conſequence was that the camphor † left the ſpirit and united with the copal to form one ſolid coherent maſs at the bottom of the veſſel; the thickneſs of this maſs prevented the heat from penetrating to the ſpirit ſo ſoon as it otherwiſe would have done; and, when at laſt it did boil, the camphor had entirely left it, and of courſe it was impoſſible that any ſolution ſhould take place.

—and why Mr. S.'s friend failed.

Three times was the ſolution attempted in this manner, but when on a fourth trial by myſelf a ſufficient quantity of camphor was added and the boiling heat immediately applied, the ſolution went on, but not ſo favourably as in my other attempt, becauſe the copal was united into one maſs at the bottom of the veſſel, and no more could be expoſed to the action of the ſolvent than the ſurface; but, in the other experiment the copal was broke into very ſmall bits and the ſurface of each expoſed to the full action of the ſolvent; of courſe a greater quantity was diſſolved in the ſame ſpace of time.

The inſtance of failure here mentioned might have been urged againſt the proceſs.

Had this gentleman been ſatisfied with the experiment he tried by himſelf, he might have reported, from his own experience, that copal could not be diſſolved by the proceſs I had deſcribed; and yet in point of fact, nothing could have been more untrue: from this it may be fairly deduced that when perſons who are unuſed to ſuch experiments do undertake them they ſhould be ſcrupulouſly exact in following the directions

* Six months ago I reduced ſome copal to this ſtate; it has remained ever ſince expoſed to the open air in a hot room, and ſtill remains ſoft, the particles adhering to each other.

† When the ſpirit of turpentine was poured off from the copal, it was perfectly colourleſs, and by the ſmell did not in the leaſt indicate the preſence of camphor.

given

given to them, and on any apparent failure, reſort to thoſe whoſe experience may enable them to detect any fallacy there may be in the proceeding.

As it is well known that ſolution of copal is the moſt valuable of thoſe varniſhes which can be uſed for the finer works of art, and as it is proved that by the proceſs I have deſcribed, theſe ſolutions may be eaſily made by any one in almoſt any ſituation, it only remains to mention ſome cautions which are neceſſary to enſure ſucceſs. Remarks and cautions.

Firſt, That the ſpirit and camphor ſhould be united before the copal is added.

Secondly, That the copal ſhould be groſsly powdered before it is added to the ſpirit, the whole ſhould then be made to boil as *quickly as poſſible*, but not *violently*, and that ebullition ſhould be kept up regularly till the ſolution be compleat.

There ſeems to be no diſadvantage in uſing too much camphor if it ſhould ſo happen; but it *is* diſadvantageous to put in too little copal; becauſe a certain quantity of ſpirit will take up a given quantity of copal; and if more is added than will be diſſolved, the ſuperabundant copal will abſtract more camphor from the liquid, and thus render the ſolution more tedious and difficult, and ſometimes, perhaps, entirely prevent it. Effects of an overdoſe of camphor or of copal.

The advantage that attends the uſe of glaſs veſſels is that the ſolution may be ſeen all the time it is going on; the diſadvantage is that perſons who are not accuſtomed to ſuch purſuits, may inadvertently give too much heat, and thus ſubject themſelves to ſerious accidents; but by uſing veſſels of metal with the precautions I have given, no accident can happen; the only diſadvantage is that they cannot ſee what they are doing; this is of ſmall conſequence, as it may always be determined with ſufficient accuracy by the ear whether the ſpirit boils, and to what degree; and as no accident can happen, without very groſs neglect, from the uſe of them, they certainly ſhould be preferred. Glaſs veſſels and thoſe of metal compared.

As I have written more than I at firſt intended, I ſhall conclude with a requeſt that you will uſe your diſcretion in omitting any part or rejecting the whole of what I have written, if you think the publication will not anſwer any uſeful purpoſe. I ſend with it a ſmall quantity of copal diſſolved in alcohol, and another Concluſion.

another in ſpirit of turpentine, which will enable you to ſatisfy yourſelf and your readers whether theſe ſolutions *deſerve to be placed in the claſs of varniſhes,* and remain

Your moſt obliged ſervant,

T. SHELDRAKE.

No. 5, Montague-Street,
Ruſſel-Square.

IV.

Reply to Mr. DALTON, *on the Conſtitution of Mixed Gaſes. By Mr.* JOHN GOUGH.

To Mr. NICHOLSON.

SIR, *Middleſhaw, Oct.* 16, 1804.

Character of Mr. Dalton's reply.

I DID not attack Mr. Dalton's opinions openly, until he had invited me to do it; the attempt has been made on my part purſuant to his requeſt; and he has replied, but in a manner which ſeems better calculated to amuſe the ſuperficial reader, than to convince the reaſoner: For he treats the ſubject, ſometimes with acrimony, and ſometimes with ridicule; in the mean time his arguments are but few, and theſe appear to be negligently conducted. A bare inſpection of his own letter will ſupport the two firſt charges, and I will now enter upon the defence of the third.

Mr. Dalton begins by pronouncing the diſtinctions which I make betwixt a mechanical hypotheſis and theory, to be nugatory; notwithſtanding the cauſes of theſe diſtinctions are fully ſtated in my Strictures. Now controverſy is of a nature which always obliges a man to point out the fallacy or futility of his opponent, when he can do it with ſucceſs; but this has been neglected by Mr. D. in relation to my diſtinctions, and I leave him to draw the inference which reſults from theſe obſervations.

Mr. D.'s demonſtration reviewed.

Mr. D. proceeds, in the next place, to demonſtrate the fundamental propoſition of his hypotheſis; namely, the mutual penetrability of gaſes which do not attract nor repel each other. For this purpoſe he remarks, that oxigen repels oxigen, but not

not azote. This aſſumption is fairly allowed to be a poſtulate by Mr. Dalton; and he then proceeds to obſerve, that this being admitted, it follows, that if a meaſure of oxigen be put to one of azote, the oxigen finding it, *viz.* the azote, porous, enters its pores, and *vice verſa*, &c. But here a ſecond and an eſſential poſtulate is ſlily introduced into the ſyllogiſm, under the form of an inference; becauſe no ſtrict logician will venture to ſay that a body, A, muſt be porous of neceſſity, becauſe it repels a body B; on the contrary he muſt conclude, that this miſtake in his art deſtroys the demonſtration, and does not retrieve the doctrine from the imputation of being an hypotheſis. The ſimile of the philoſopher, cottager, and ſieve, may be calculated to promote ridicule, but it is badly choſen; for it is impoſſible to demonſtrate the propoſed problem from Euclid's Elements, becauſe this work confines its ſpeculations to abſtract figures and magnitudes. But a mathematician might demonſtrate the ſame to a brother mathematician, from the elements of the mechanical philoſophy; and he ought to have his demonſtration ready upon demand, which is not the caſe of Mr. D. in the inſtance of his fundamental propoſition.

Mr. D.'s ſimile incorrect.

It is a matter of ſome ſurpriſe, that Mr. D. has drawn his demonſtration from the mutual affections aſcribed by him to oxigen and azote; ſeeing I had profeſſedly attempted to ſhew the fallacy of his ſecond and eſſential poſtulate, by aſcribing the ſame affections to aqueous vapour and the permanent gaſes of the atmoſphere. This unfortunate choice obliges my opponent to be content with a diſtant ſide-view of the arguments which have been brought againſt him; whereas a ſtricter obſervance of the path* chalked out for him in my Strictures, would have given him an opportunity to confront them, and defend his atmoſphere of ſteam. The thing however has turned out, for ſome reaſon or other, as it is here repreſented; and Mr. D. arrives very briefly at the following ſingular concluſion; that though the demonſtration of my propoſition reſpecting air and vapour, be rigid, the previous data are not correctly aſſumed. Now this is the point which I endeavoured to eſtabliſh in the propoſition alluded to above; for his ſecond poſtulate makes one of the incorrect data; in fact, they are all

The ſecond poſtulate falſe in the inſtance of vapour and air.

* Phil. Journ. Vol. IX. Page 53.

his own except this: If a particle of vapour can paſs freely through the air, a ſecond can alſo ſucceed it at any given diſtance. My antagoniſt will certainly grant me this conceſſion; becauſe the ſalvation of his aqueous atmoſphere obliges him not to ſuſpect it. Should this indulgence be extended to my Strictures, the demonſtration in queſtion muſt be re-examined with greater diligence by Mr. Dalton; for if it cannot be refuted, it will manifeſtly do away his atmoſphere of free vapour, by proving the abſurdity of his ſecond poſtulate in the caſe of air and water; and the ſame obſervation may alſo be extended to all the permanent gaſes which are not abſorbed by water; becauſe no fluid of this deſcription forces water to paſs through its pores by preſſing upon it.

The uſe of the contemplative analyſis. After all, if we may judge from Mr. Dalton's reply, he appears not to comprehend my arguments on this ſubject: they have however nothing of novelty in them; for the beſt reaſoners in all ages have not heſitated to admit a hypotheſis is for the ſole purpoſe of diſcovering its worth, by comparing the conſequences reſulting from it with known facts. This kind of argumentation was in great eſteem with the Greek mathematicians, and Pappus of Alexandria has deſcribed and recommended this ſpecies of logic under the name of the contemplative analyſis.

Facts oppoſing the hypotheſis. Mr. Dalton ſays, he is acquainted with no facts that contradict what I call his experimental probabilities. I can furniſh him with one to which perhaps he is a ſtranger, and remind him of another, which he will not diſpute: it will therefore be his buſineſs to reconcile them to the hypotheſis of an aqueous atmoſphere.

An experiment by Mr. Kirwan. *Firſt,* Mr. Kirwan, in the courſe of his ſtatical experiments on air, conſtantly found a given bulk of it to be lighter, *cæteris paribus,* when De Sauſſure's hygrometer was at 90°, than when the atmoſphere was leſs humid. Now according to Mr. D.'s hypotheſis, all the permanent gaſes, of this given bulk, had the ſame weight both in the dry and moiſt ſtate of the air; becauſe they were of the ſame denſity on account of the *cæteris paribus:* but the aqueous vapour was moſt abundant when the weight of the aggregate was leaſt; that is, the weight of vapour diminiſhes, while its denſity increaſes. Will Mr. D. admit the truth of the preceding concluſion, to the detriment of his own doctrine; or will he refute the inference, by denying the fact?

Second, If the latter alternative ſhould be preferred, the following experiment, with which Mr. D. is already acquainted, remains to be explained by the principles of his hypotheſis, due attention being at the ſame time paid to the axioms of Dynamics. An experiment with a moiſt bottle.

A moiſt bottle, which I had found would contain 7794 grains of water of the temperature of 59°, was placed in water heated to 126°, having its mouth elevated about two inches above the ſurface. This aperture was covered by my hand; which was occaſionally removed for an inſtant, that the warm air might eſcape. At the end of two minutes the bottle was inverted in cold water, the mouth of it being firſt ſecured in the manner deſcribed above; in which ſituation the temperature of it was again reduced to 59°. The air remaining in it after refrigeration, occupied 6172 of the 7794 parts conſtituting the capacity of the bottle. From this fact I infer, that 6172 parts of air of 59°, will occupy 7794 ſuch parts after being raiſed to 126° in contact with moiſt glaſs. But 6172 parts of dry air of 59°, will only occupy 6992.C6 parts, after being heated to 126° in a dry tube: Thus it appears that the preſence of water augmented the bulk of the air which I uſed; how is this difficulty to be explained? We have no ſtopple of mercury in this experiment, by help of which Mr. D. explains the appearances of the manometer; conſequently, as nothing was in the way to prevent the free egreſs of the vapour, it expanded itſelf in the bottle, and acted on the air contained in the ſame, juſt as it does in the atmoſphere. But it increaſed the bulk of this air, which could not be effected on Mr. D.'s principles, otherwiſe than by diſtending the pores of it. Now his own tables of the force of vapour will ſhew this to be impracticable, if there be any truth in the axioms of Dynamics. On the contrary, nothing occurs in the two laſt experiments repugnant to chemical union; and as water makes a part of the atmoſphere, it muſt be attached to air by the mediation of affinity.

I have now done ſomething more than anſwer all Mr. Dalton's objections: the demonſtration of his fundamental propoſition has been ſhewn to be no demonſtration at all; his ſecond poſtulate has been proved to be falſe, by his own data, in the caſe of air and vapour; laſtly, two facts have been advanced which are inexplicable by my opponent's principles: Recapitulation.

ciples: After perusing these objections, can he blame me for rejecting his hypothesis. As for Mr. D.'s remarks on my theory of mixed gases, they require no answer; because he professes not to understand it. I am willing, however, to submit the merits of it, as well as its claim to the title of a theory, to those mechanical philosophers who happen to comprehend it.

JOHN GOUGH.

V.

On the apparent Size of the horizontal Moon. In a Letter from Mr. EZEKIEL WALKER.

To Mr. NICHOLSON.

DEAR SIR,

Enlarged appearance of the horizontal moon.

WHEN the full moon is just rising above a clear horizon, she is an object that pleases every eye: But at the same time that she pleases the eye of the philosopher, she embarrasses his reason, to assign the cause of her apparent magnitude's being so much greater near the horizon, than at higher elevations.

Attempts to account for it.

When men in former ages began to reason on this phenomenon, they imagined that the angle subtended by the moon, was really increased, by a refraction of her rays, in passing through the vapours contained in the air, near the earth's surface. But when it became known that she subtended the least angle at the eye, when her apparent magnitude was greatest, philosophers said that the eye was imposed upon by the long series of objects interposed between the eye and the extremity of the sensible horizon. And after it was discovered that the same phenomenon was observed at sea, where no land objects could be seen, they blamed the clouds for deceiving them; and when the clouds flew away, the spirit of inquiry flew after them, to seek for information in the apparent concavity of the sky.

Such were the erroneous opinions maintained by men of the greatest celebrity, both in ancient and in modern times. To mention any more of them would be useless labour: a single quotation from Dr. Smith's Optics will, I presume, be quite sufficient to show how little was known of this matter at the time when he wrote.

After

After the Profeſſor had finiſhed his celeſtial explanation of this phenomenon, he juſtly acknowledges that, at different times, the moon appears of very different magnitudes, even in the ſame horizon, and occaſionally of an extraordinary large ſize, which he is not able to give a ſatisfactory explanation of. *Smith's Optics*, Vol. I. pa. 63, &c. *Remarks*, pa. 53.

It is really aſtoniſhing, that this phenomenon ſhould have remained ſo long without an explanation founded on a better principle than that of mere opinion. That the dimenſions of the pupil of the eye alter by the ſtimulus of light, has long been known; but I believe it ſtill remains to be proved, that the picture of the moon formed upon the retina is not permanent, but varies as the dimenſions of the pupil vary. The following experiments will, I hope, remove every doubt reſpecting this law of viſion.

Explanation offered upon the poſition that the images on the retina vary with the ſize of the pupil.

To imitate the eye upon a large ſcale, I took an achromatic lens of 1.6 inches in diameter, and 17 inches focal diſtance, to repreſent the cryſtalline humour. This I fixed upon a ſtand in a perpendicular direction, with a moveable white ſcreen behind it for the retina, to receive the image of a lighted candle, which ſtood before it, at the diſtance of 40 inches. To imitate the pupil, I took three cards, and numbered them 1, 2, and 3, In No. 1, I made a circular hole half an inch in diameter, in No. 2 a hole of $\frac{3}{10}$ of an inch, and in No. 3, a hole of $\frac{2}{10}$ of an inch.

Experiment. A candle was placed before an achromatic lens, and three apertures were provided.

After I had moved the ſcreen until the image of the candle appeared diſtinctly upon it, I found that the length of this luminous picture meaſured 1.82 inches.

Focal image by the whole lens 1.82 inches long.

When the card No. 1, was applied cloſe to the lens, with the centre of the hole againſt the centre of the lens, the inverted image of the flame of the candle, upon the ſcreen, meaſured 1.46 inches.

Half an inch aperture gave an image of 1.46 inch.

When the card No 2, was applied to the lens, in the ſame manner as No. 1, the image of the flame meaſured 1.3 inches.

Three tenths gave 1.3 inch.

And with No 3, the picture of the flame meaſured only 1.2 inches.

Two tenths gave 1.2 inch.

Theſe experiments ſhow us, in the cleareſt manner, the cauſe of that variation which obtains in the apparent magnitudes of objects.

Application of the experiments to the moon and other objects,

That

That the pupil contracts as the quantity of light which falls upon the eye increases, is well known.

And that the magnitude of the picture of a luminous object upon the retina, decreases as the pupil contracts, is evident from the experiments.

Therefore, the magnitude of the picture upon the retina, decreases, as the quantity of light falling upon the eye increases.

Hence the apparent magnitude of the moon is greatest at the horizon, and decreases as she ascends; for the magnitude of her picture upon the retina is inversely as the quantity of light which she gives us.

and to the sun. The sun appears larger at the horizon than at higher altitudes, for the same reason; and terrestrial objects seen through a mist appear larger than in a clear day, in consequence of the same operation of the eye.

I am, Dear Sir,
With much respect,
Your humble Servant,
E. WALKER.

Lynn, Oct. 13, 1804.

VI.

Description of the Ship Economy, 200 *Tons Measurement, built on the improved Construction of Mr.* J. W. BOSWELL. *

THE plan adopted in the formation of this ship is that designed for large ships of 500 tons and upwards, and the third mentioned in my specification †.

Its external appearance is nearly the same as that of any other vessel of the size, and the outward planking done in the usual manner. It is the internal construction alone to which the patent relates, and that is as follows:

Description of a vessel framed in a new method. The best general idea of it will be obtained by conceiving a vessel built with timbers, or ribs, much smaller than usual, with

* For which he has a patent.

† See Vol. II. Second Series, p. 81, of Repertory of Arts.

an

an internal framing, ſo contrived as to give every requiſite ſupport and ſtrength both to them and the entire veſſel, with the leaſt timber, and of the cheapeſt form, and without any knee-timber.

Deſcription of a veſſel framed in a new method.

The floor-timbers are molded ſeven inches, and ſided ſix: theſe, with four futtocks and two top timbers at each ſide, form what is called a frame of timbers. Thoſe ſmall timbers are laid down ſo that their terminations all fall out in fair lines, which are nearly the ſame as the ribband lines, when below the wales. Along thoſe lines inſide are laid fore and aft ribs, from ſtem to ſtern poſt, ſo as to ſupport the extremity of every one of the ſmall ribs in the ſhip. The fore and aft ribs are ſix in number at each ſide; one directly under the water ways, another at the level of the lower beams, and the other four placed nearly at equal diſtances between theſe laſt and the kelſon: each pair uniting in a breaſt-hook at the ſtem.

The pieces of timber which form theſe fore and aft ribs are ſcarfed at their extremities with hook ſcarfs, and ſo placed that the ſcarfs fall out in fair vertical ſections of the ſhip, where they are ſupported, and firmly bolted to tranſverſe framings, contrived ſo as to unite the greateſt ſtrength with the leaſt obſtruction, and which are five in number in the whole ſhip.

Thoſe tranſverſe framings laſt mentioned muſt be conſidered as the great ſupport of the veſſel, and the foundations, as it were, on which all the other parts reſt, as the beams of a wooden bridge are ſupported by the piers. Thoſe tranſverſe framings are each formed by one upper and one lower beam, two pair of futtocks, a floor timber, two pair of top timbers, and four bracing pieces; the whole connected into one firm framing, ſelf-ſupported, independent of any other part.

The four bracing pieces form each framing into a ſet of triangular compartments: which triangular framing gives the greateſt ſtability poſſible, as a triangular frame cannot be made to give in, or alter its figure, by any force which is not ſufficient to tear its connecting parts through the timber of which it is compoſed; a property which no other figure poſſeſſes.

The leading principle is that triangular framings cannot vary.

Theſe tranverſe framings (beſides ſupporting the fore and aft ribs, and by them the ſmall vertical timbers) tie and unite the veſſel together acroſs ſhip, ſo as to give much greater ſtrength than hanging-knees, whoſe place they ſupply, at a much cheaper rate.

The

Framing of the deck.

The framing of the deck is alfo divided into triangular compartments, as fpecified in my patent, as to preclude the ufe of lodging-knees entirely; which compartments are formed by fix pieces of timber, which proceed obliquely at each fide, from the top of each beam to the fore and aft rib next adjoining, into which they are dovetailed and bolted; long carlings from beam to beam, at each fide of the hatchways, with thefe pieces, fupport fmall ledges, on which the deck is laid in the ufual manner.

The veffel in coming round from Southampton water failed remarkably faft, and ftayed and fteered admirably well.

Advantages of this Method of framing Ships.

Advantages of the method:

1ft. Timber of lefs than one fourth of the ufual girth can be ufed, in this method, in conftructing large veffels, for nearly four-fifths of their frames.

in price of timber;

This will be a direct faving in the difference of price of fmall timber and large for the quantity ufed; for large veffels, this will be confiderable, and, according to the prefent contract prices for naval timber, not lefs than from two to four pounds per load. Befides this, it is a great national benefit in another point; for, by this means, timber of half the number of years growth, or lefs, can be ufed for naval purpofes; and thus forty or fifty years, or even lefs, be fufficient to produce timber fit for the navy, inftead of the vaft period of near a century, now neceffary; by which the land will not only produce a double crop in the fame time, fit for this purpofe, but all danger be removed of there being a ftoppage of building, for want of a fupply of timber, at any future period; an event extremely probable to take place, from the increafing difficulty of getting the large kind ufed at prefent in the Royal Dock Yards.

ufe of fhorter pieces, &c.;

2d. Much fhorter timber may, in forming the futtocks, be ufed, without any danger of weakening the fhip, on account of the great fupport given to them by the fore and aft ribs, and other internal framing, before defcribed.

The advantage of this is, that it renders the compafs timber for futtocks eafier to be procured, and prevents any neceffity of ufing any timber cut acrofs the grain.

knees fuperfeded;

3d. The ufe of knees of every kind is fuperfeded by this mode of building, as the triangular framing of the decks gives

5 all

all the effect of lodging-knees, and that of the tranfverfe frames more than fupplies the fupport given by hanging-knees.

This would occafion a confiderable faving in large veffels, on account of the great price of knee-timber fit for them; which, for that of 30 feet meeting, is near ten pound per load and for the fmalleft kind, taken at the dock-yard, not lefs than 8*l.* 15*s.*

4th. Plank of half the ufual thicknefs may be ufed for lining; the great fupport given by the fore and aft ribs rendering any ufe of infide plank, to ftrengthen the veffel, needlefs, and confining its purpofe merely to prevent ballaft, or other matters, from getting between the timbers, fo as to reft on the outfide plank. **thinner planking;**

This will alfo caufe a faving of confequence in large veffels; plank of all kinds, but particularly that of great thicknefs, being the next deareft article to knee-timber.

5th. It is probable a much lefs quantity of timber might be ufed with fafety in this method, on account of the great ftrength produced for the timber ufed. 1ft. By the triangular framing. 2d. By every timber having a folid fupport at each extremity. 3d. By the increafe of thicknefs from in to out all along the fore and aft ribs, being very great in proportion to the timber ufed. **lefs quantity of materials;**

6th. It is probable, veffels built in this method will laft many years longer before decay; becaufe the ufe of fmall timber admits of a kind more fpiny and durable than the large, which is often dotard, and never lafts fo long; and alfo becaufe this conftruction admits of a free circulation of air among the timbers, than which nothing is known to contribute fo much to their prefervation. It is moreover conceived, that the timbers being prevented from working by the folid fupport each has at its extremities, will caufe the veffel to wear lefs, and at the fame time render it fafer, by diminifhing the danger of ftarting planks, or otherwife caufing bad leaks. **durability.**

7th. The timber of confiderable fize ufed in this method is almoft all nearly ftraight, or of very little curvature, on account of its running fore and aft.

This kind is much eafier to procure than large compafs timber.

8th.

8th. Short-top timber and coarfe butts can be worked up to advantage, inftead of being fold for lefs than half coft, or burned; as this kind will do fufficiently well for the number of fhort ledges in the deck frames, and to fupport the lining at the floor, which are wanted in this mode of building.

9th. Veffels built in this manner will not be fo liable as others to hog, or have their backs broken, on account of the great ftrength length-ways, caufed by the fore and aft ribs.

10th. Veffels fo built will be drier, from the circulation of air before mentioned, and having the floor-lining detached from the timbers; which quality renders this conftruction particularly valuable for the fhips ufed in the Eaft and Weft India trade.

The advantages above recited relate to veffels entirely formed in this manner. It fhould be known alfo, that parts of this plan may be applied with profit. The mode of framing the decks, for inftance, might be ufed to fave lodging-knees in veffels built in other refpects in the ufual mode. Other parts of it might be applied to the ftrengthening old veffels, which, by this means, might be made to laft many years, after they would otherwife have been unferviceable.

The principles of this method of building are capable of being extended ftill further than they are in the veffel here defcribed: the triangular framing may be even adopted to the conftruction of fore and aft ribs, fo that thefe could alfo be conftructed of fmall timber, if required. Thus, by this means, the former barrier to the increafe of fize in fhips is removed, as it no longer now depends on the fize of timber; and fhips of any dimenfions required may be formed, of any ftrength requifite, of fmall timber.

J. W. BOSWELL.

Concluding

VII.

Concluding Remarks on the Computation of Tables of Squares and Cubes. In a Letter from E. O.

To Mr. NICHOLSON.

SIR,

I AM ſorry to trouble you again on a ſubject which I fear can afford but little intereſt to the generality of your readers; but I muſt requeſt the admiſſion of a few remarks upon the letter which you publiſhed in your laſt number from H. G.

On the computation of ſquares and cubes.

Your correſpondent is certainly miſtaken when he aſſerts that our methods of calculating ſquares are "preciſely the ſame:" Both, indeed, are derived from the principles of the binomial theorem; but I conceive that there is a material difference in our manners of applying thoſe principles to practice. As we find the ſquare of the given number by adding a certain quantity to the known ſquare of the number next below it, it follows, that in both methods the quantity ſo added muſt be the ſame; but there is a conſiderable difference in the operations by which we find this number: I do it by the repeated addition of the ſmall number 2, whereas H. G. finds the ſame quantity by the repeated doubling of a number, which is conſtantly changing, and may poſſibly conſiſt of many figures. To ſhew the difference more clearly, I will calculate ſeveral ſquares according to the direction given in P. 150, Vol. VIII. and I will take for my example the ſame which I have taken in P. 6, Vol. IX. that your readers may the more eaſily form the compariſon for themſelves.

$$\begin{array}{rcl} 28261 & & \\ 2 & & \\ \hline 56522 & = & 2.28261 \\ 798684121 & = & \overline{28261}|^2 \\ 1 & & \\ \hline 798740644 & = & \overline{28262}|^2 \end{array} \qquad \begin{array}{rcl} 28262 & & \\ 2 & & \\ \hline 56524 & = & 2.28262 \\ 798740644 & = & \overline{28262}|^2 \\ 1 & & \\ \hline 798797169 & = & \overline{28263}|^2 \end{array}$$

28263

On the computation of squares and cubes.

$$\begin{array}{rl} 28263 & \\ 2 & \\ \hline 56526 & = 2.28263 \\ 798797169 & = \overline{28263}|^2 \\ 1 & \\ \hline 798853696 & = \overline{28264}|^2 \end{array} \qquad \begin{array}{rl} 28264 & \\ 2 & \\ \hline 56528 & = 2.28264 \\ 798853696 & = \overline{28264}|^2 \\ 1 & \\ \hline 798910225 & = \overline{28265}|^2 \end{array}$$

Perhaps it may be objected to my ſtatement, that the calculator would be aware that $28264.2 = 28263.2 + 2 = 28262.2 + 4$, &c. or that he muſt ſoon diſcover this law of continuation; and therefore, that he would not proceed in every inſtance to the actual multiplication of the root by 2. Such an objection, however, will avail nothing againſt my argument, for in that caſe he will no longer work by the rule given by H. G. in P. 150, Vol. VIII.; but if he proceeds to find the difference by addition, he muſt virtually purſue the method which I recommend; the only difference will be, that the practice of calculation will have ſuggeſted to him an improvement which he might have originally derived from firſt principles.

I ſhall not enter into the queſtion, whether addition is, or is not, a ſimpler proceſs than ſubtraction. The preſent queſtion may be decided independently of it. For granting that there is no reaſon to prefer one to the other, yet the two methods by which we find the firſt differences of the cubes, might be fairly put to iſſue upon the number of figures uſed in each. To find them according to the method recommended by H. G. in p. 150, Vol. VIII. we muſt ſubtract the whole of one cube from the whole of the other; whereas I find them by the conſtant addition of the ſecond differences, which muſt neceſſarily be much ſmaller numbers. Thus to find the firſt difference between the cubes of 26561 and 26560:

According to H. G.	*According to E. O.*
18738432796481	2116221121
− 18736316416000	+ 159360
2116380481	2116380481

It is ſtated, indeed, by H. G. that he avoids the continual repetition of firſt differences. By this we muſt not underſtand that

that his method makes it unneceſſary to calculate them; for by mere compariſon of p. 9 with p. 125, it will be ſeen that we both uſe the ſame numbers. In this inſtance, as in that of the ſquares, the real difference between the two methods conſiſts in the manner of finding theſe additional quantities. H. G. muſt therefore mean, that he avoids the neceſſity of calculating the table given in p. 8. Now in this I do not think that any advantage is gained. For if the firſt differences are to be found ſeparately, I have ſhewn that they can be more eaſily found by the method which I have recommended; and much expedition will be obtained by making a ſeparate table of them, which is impoſſible if we work according to the directions of H. G. I may, with ſome reaſon, complain, that H. G. has not made his example a fair parallel to mine; for I have ſet down every figure which need be uſed, while he has declined printing any of the operations whereby he determined his firſt differences. It is poſſible, indeed, from long practice in arithmetical computations, that he may be able to caſt his eye from one cube to the other, and ſo find the firſt differences, without copying them out on the ſide of his paper for ſubtraction. I ſhall not dwell upon the danger of miſtake in ſuch a proceſs: the quickeſt and moſt accurate eye could hardly avoid frequent errors, and the common arithmetician will find it attended by conſiderable difficulty. But granting that all this difficulty could be completely conquered, ſtill it would give no ſuperiority to the method recommended by H. G.; for it might be applied to mine with equal, if not greater eaſe and advantage. Thus, in the example given in p. 125; if F were computed by the addition of C and D, inſtead of the ſubtraction of A from E; if we conſider $I = F + G$ inſtead of $= H - E$, $M = I + K$ inſtead of $= L - H$, &c. we ſhould have the differences computed according to my rules. We may remark likewiſe, in this ſhorter method of calculating, that, according to the arrangement given by H. G. himſelf in p. 129, the eye muſt neceſſarily paſs over two intermediate lines of figures in ſubtracting one cube from the other, whereas the two rows of figures which I would have added to one another, may always be placed contiguous. This certainly is an advantage; but at the ſame time I only mention it to ſhew the grounds

On the computation of ſquares and cubes.

On the computation of ſquares and cubes.

grounds on which I prefer my method to that of H. G.; for I ſtill conſider the plan which I firſt laid down as the eaſieſt and ſureſt. . .

I have ſaid, that if the differences are conſidered in p. 125 as found by addition inſtead of ſubtraction, the example would ſuit with my method, becauſe the ſecond differences are ſtill additional, and will be found, upon compariſon, to be added in exactly the ſame order as in pp. 8 and 9. The only variety is by making two tables: I add the ſum of the two numbers which are ſeparately arranged under each cube. I have only to add on this part of the ſubject, that H. G. has adopted my method in ſome meaſure; for he ſays that K = G + 6, as well as = 6*d*. It is true (as I remarked with reſpect to the ſquares, that this equality would moſt probably occur to the calculator; but at the ſame time we muſt recollect, that it is different from the rule laid down in p. 150, Vol. VIII.

I ſhall not add any thing to what I have ſtated in p. 5, with reſpect to the miſtakes to which either method is liable; but I ſhall conclude by aſſuring you, that it is not my intention to trouble you any more on the preſent ſubject. I thought it might be uſeful to publiſh the letter which I firſt ſent you, and having done ſo, it became neceſſary to remove any obſcurities in it, and to anſwer any miſtaken objections which might be raiſed againſt it: but this having once been done, there can be no occaſion for further controverſy; and I have only to thank you for the indulgence which you have granted me.

I am, Sir,

Your obliged humble ſervant,

E. O.

P. S. I take this opportunity to beg you will mention two or three preſs errors which occur in my former letter.

P. 9, line 20, dele the Italic *e*.

P. 10, line 12, $\overline{x+1}|^3$, read in both inſtances, $\overline{x+1}|^2$

P. 12, line 16, 29 read 27.

Letter

VIII.

Letter from C. WILKINSON, *Esq. on Galvanism and Electricity.*

Dublin, Oct. 8, 1804.

To Mr. NICHOLSON.

SIR,

WHEN at Liverpool a few days since, at the Athenæum I found a paper of Mr. Thicknesse of Wigan in your valuable Journal, and which has induced me to trouble you with the following observation. Mr. T. ingeniously conjectures that the galvanic phenomena depend more upon the decomposition of the water employed than as to any chemical change effected on the metals. Mr. T. observes that two metals are requisite to the production of galvanic phenomena: this is no ways the case, it is well known that a single metal suffices, or even brain and muscle, according to the experiment of La Grave, or nerve and muscle according to Aldini. Mr. T. further observes that hydrogen mixed with copper always renders it brittle. I should wish to know this gentleman's authority for such an assertion: he also sets out too hypothetically as to electricity being a modification of caloric. It has always appeared to me that the galvanic phenomena entirely depend upon the disengagement of electricity from the metal undergoing a chemical change. Observations on galvanism, &c.

Galvani has ascertained that gold, silver, copper, iron, tin, lead and zinc, constitute the series of metallic bodies; that when two metals the most remote in the series are united, the most powerful galvanic combination is formed; thus gold and zinc, silver and zinc, copper and zinc, &c. and lastly, lead and zinc, form the weakest galvanic combination. The disposition to oxidation is in the inverse order, thus zinc will become oxidated even by exposure to the air, whilst silver and gold undergo this change with the greatest difficulty.

Hence a metal which is oxidated with the greatest difficulty combined with a metal which oxidates with the greatest facility, form the most powerful galvanic combination. The production of galvanic phenomena is always proportionate to the degree of oxidation.

All

Obfervations on galvanifm, &c.

All fubftances which are conductors of electricity, if they undergo any change as to their conducting power, fo as to become diminifhed as conductors, always in the change lofe a portion of their natural electricity. Thus metals which are the beft conductors, when oxidated, become non-conductors in this change, the combined electricity is loft.

If a plate of zinc is univerfally immerfed in a fluid which will produce chemical changes upon it, no galvanic phenomenon will be produced becaufe the metal in every affignable point, being equally acted on, the contrary ftates of electricity cannot then be produced.

If only one fide be acted on, every affignable point on that fide undergoing a chemical change, by which it is difpofed to part with its combined electricity, there will be a general tendency of the electricity in the fubftance of the metal towards the furface acted on; the fame as if in a veffel filled with water, a fmall aperture be formed, there will be a general tendency or current of the water towards the aperture, fo of electricity.

Dr. Wollafton has proved by very ingenious experiments, that when two diffimilar metals are placed in a fluid which will act upon one of the metals and not upon the other, when the two metals are put into contact in the fame fluid, that thus chemical changes are effected on both; thus that gold and filver thus arranged, will be acted on by the nitric acid.—When two metals are placed in a fluid which will act upon either of them feparately, if the two metals are preferved in a ftate of feparation while in the fame fluid, chemical changes will only be effected upon one. Thus zinc and copper placed in nitrous acid and water, only the zinc will be acted on.

Thefe principles point out the neceffity in our galvanic troughs to have our cells perfectly infulated, and that there be no communication between the refpective plates.

Mr. T. has by miftake obferved that the fenfation is in proportion to the furface acted on; the experiments of the French philofophers proved that the action of galvanifm on animal fubftances is in the ratio of the number of plates employed, and not the furfaces expofed.

In all my experiments in town with my own electric battery I never fucceeded in giving the flighteft charge with my galvanic apparatus. In fome converfation I had with you, Mr. Editor, you conjectured that an extenfive feries of fmall jars would

would be the beſt arrangement. When at Liverpool, I met with ſuch at the houſe of Mr. Dalton, a very ingenious lecturer on electricity. The galvanic battery I employed conſiſted of 200 eight-inch plates, which had fuſed at the time near five feet of ſteel wire; this communicated a very ſlight intenſity to the electric battery, containing forty feet coated ſurface, juſt ſufficient to convulſe a frog, and although the electrical battery was in the beſt ſtate of preparation, we could not ſucceed in producing any intenſity as even to affect the tongue. At another period, I ſhall do myſelf the pleaſure of tranſmitting to you an account of ſome ingenious experiments of Mr. Dalton, on electricity, he finds a cylinder, when exhauſted of air, or when one atmoſphere is condenſed into it, will not then be capable of excitation.

I am, Sir,

Your's ſincerely,

C. WILKINSON.

No. 19, Soho-Square.

IX.

Method of preventing Accidents to Horſes and Carriages, in going down Hills, by a Gripe or Claſp acting on the Naves of the Wheels of the Carriage. By Mr. W. Bowler.*

SIR,

THE invention I have now ſent you is, I think, likely to be of great uſe, and I therefore offer a model thereof for your inſpection. In the firſt caſe, this cart may be ſtopped in an inſtant, in going down the ſteepeſt hill with a load, without ſtopping the horſes, by the carter only preſſing his hand upon a lever. This plan would likewiſe be highly advantageous in caſe of a horſe taking fright, as the carriage may be inſtantly ſtopped by the brace claſping the wheels. It may alſo, with little alteration, anſwer for a broad-wheel waggon with a heavy load. By the preſent method, a coachman, whilſt ſit-

Apparatus to lock the wheels of carriages.

* Tranſactions of the Society of Arts. A bounty of ten guineas was given to the inventor, and the Society have a model.

Apparatus to lock the wheels of carriages.

ting on his box, may confine or releafe the wheels of his carriage at pleafure, and prevent accidents in defcending hills, or in managing reftive horfes.

I am, Sir,

Your humble Servant,

WILLIAM BOWLER.

Finfbury-Street, May 13, 1803.

To Mr. Charles Taylor, *Sec.*

Reference to the Engraving of Mr. William Bowler's Gripe for the Wheels of Carts and Carriages, Plate X.

A B. The extent of the body of the cart. C D. The two fhafts. E. The front ladder of the cart, part of which projects over the fhafts, and is fupported on them: the other part is faftened to the upright piece F. G. The handle of the lever, fliding betwixt two uprights, and moveable on an iron bar and pivot at H, in the upright piece F. By preffing down this handle, the moveable joints I K raife upwards the lower part of another lever L, fo as to prefs againft the lower part of the nave of the wheel, and at the fame time the moveable joints prefs down the lever O, and caufe this lever to act againft the upper part of the nave of the wheel; thus compreffing the nave of the wheel betwixt a double brace, and either retarding wholly the motion of the wheel, or allowing it to move a little, as may be thought requifite. The circle of the nearer wheel is fhown by the dotted lines P; but this wheel is not added, as it would prevent this gripe from being clearly feen. The handle of the lever G produces at the fame time a fimilar effect on the further wheel R, by means of the bar of iron which croffes the cart at H, and acts in the fame manner on fimilar double joints at the other fide of the cart, as may be feen at S, where the further double gripe is fhown preffing on the nave of the further wheel. T. A fmall catch, which, by falling into the notches or teeth of the lever, when it is preffed down, holds both the gripes in one certain pofition.

When the wheels are to be fet at liberty, the catch T is removed out of the teeth of the lever; then the lever G is raifed above the hole V, and kept from fliding down by an iron pin attached to the chain U, being put underneath the lever, through the hole V.

The

The iron-work is not any inconvenience in the loading or unloading of the cart, nor occupies any room within it; and, what is of material confequence in new inventions, it may be managed by the carter with much lefs time and trouble than the common hook and drag chain. It is alfo much more fecure in its action, as it binds on both wheels at once, and gives a uniform fteady draught for the horfes, which prevents them from falling down.

X.

Obfervations and Experiments to elucidate the Operation of the Galvanic Power. By Mr. CHARLES SYLVESTER. *In a Letter from the Author.*

To the EDITOR *of the* PHILOSOPHIC JOURNAL.

SIR,

I HAVE for fome time read and admired your valuable publication, but have not before this time ventured a communication on any fubject. If you think the following thoughts and experiments worthy the attention of your readers, I fhall feel myfelf honoured if you give them a place in your Journal.

I remain, Sir,

Your obedient fervant,

CHARLES SYLVESTER.

Sheffield, Oct. 16, 1804.

The decompofition of water, or at leaft, the prefence of oxigen and hidrogen, is obferved in all the modes by which the galvanic energy is excited. I obferve that fome of your correfpondents are inclined to doubt the truth of water being a compound body, from obferving in its decompofition that the oxigen and hidrogen are given out at fo great a diftance from each other. Though thefe philofophers have more fimply accounted for fome of the phenomena by making water a fimple fubftance, their affumed data, that pofitive and negative electricity are two diftinct bodies, is by far more gratuitous than the pofition, that water is a compound. Befides this hypothefis will only account for a few of the facts, while it abounds with contradiction when applied to the reft of the phenomena of galvanifm.

Whether water be decompofed in the galvanic procefs;

—or pof. and neg. electricity be diftinct bodies.

 They

They tell us of two kinds of electricity coming from the galvanic apparatus one from the zinc end, the other from the copper end: the former being positive, the latter negative.

The latter less probable.

Two distinct fluids moving in contrary directions could not possibly exist in the galvanic trough; by reason of that fluid proceeding from the zinc meeting that coming from the copper in each of the cells, and consequently establishing a constant equilibrium, without producing any effect. When the decomposition of water is only taking place in one vessel (admitting the above objection to be of no weight) the phenomena admit of an easy explanation by this hypothesis; but when two vessels are used connected by a wire arch, it cannot in any shape be adequate; for as we have in each vessel both oxigen and hidrogen gases, there must also be both positive and negative electricity; these two contrary fluids would of course have to meet each other in the connecting arch, and according to another part of this theory form sensible heat *.

The oxidation contradicts the hypothesis of two electric bodies.

The positive wire, if a base metal is always oxidated; this effect is known to be facilitated by the electrical agency. According to the principles in question, it would be retarded by the power of affinity existing between water and positive electricity. When the wires of a galvanic apparatus are brought into a metallic solution, no hidrogen is given out at the negative wire, but the metallic oxide is reduced. Now if metallic oxides be composed of water and the metal, the negative electricity would combine with the water, and hidrogen gas would be evolved—the very reverse of the fact.

My reason for being thus particular in the examination of the hypothesis of Mr. Richter and Dr. Gibbs, is to prepare the mind of the reader for some experiments which I think will contribute to the firmness of the hypothesis, which supposes water to be a compound, and that the hydrogen is carried from the positive to the negative wire by the electricity.

Variation of Dr. Ash's experiment.

When a long copper wire was connected at the end with zinc (under weak sulph. acid) hidrogen was earlier given out

The well-known experiment of Dr. Ash (with the plate of zinc and silver in a dilute sulphuric acid) I had occasion to vary in the following manner. I took a copper wire about eighteen inches long, and bent it in the middle so as to form

* They are of opinion that these two fluids combined form caloric. They also hold with Dr. Priestly that what are now termed metallic oxides are the respective metals combined with water.

two

two parallel legs, about two inches diſtant from each other. This I laid in an earthen diſh filled with dilute muriatic acid. I then brought a piece of zinc in contact with one of the ends: bubbles of hidrogen were immediately given out by that part of the copper. After bubbles had appeared for about two inches down this leg, I obſerved bubbles on the end of the other leg,—they now proceeded down the two legs till at laſt they appeared on the part where the wire was bended. I afterwards laid a piece of zinc at one end of the ſame diſh and a piece of gold at the other. I prepared a metallic arch with two pieces of wire, one of gold, the other zinc, ſoldered together. The zinc end I connected with the piece of zinc in the diſh, and the gold end with the gold. After the contact had been made about ten ſeconds, bubbles appeared on the piece of gold. The diſtance was about twelve inches. I now placed the pieces of gold and zinc at half the diſtance, and made the communication with the ſame arch. The bubbles now appeared upon the gold in about half the before-mentioned time. Upon bringing the pieces ſtill nearer together, the gold gave out gas almoſt inſtantly, but yet in all the three inſtances I had time to obſerve that the bubbles always appeared firſt on that ſide the gold next the zinc.

at the nearer parts of the (bended) wire; —and ſo likewiſe in a compound conductor between gold and zinc.

I think from the above experiments that the diſengagement of hidrogen from the gold does not depend upon its negative ſtate, for this quality muſt have been produced on the gold in every one of the three inſtances in the ſame time, from the communication being always made by an arch of the ſame length. On the contrary, it proves that the electricity diſengaged from the zinc is communicated to the gold through the intervening liquid. If we obſerve the length of time taken up in its paſſage through this medium, we ſhall ſee that it does not agree with the laws of electricity. Is it not therefore probable that when the oxigen of the water combines with the metal, the hidrogen combines with the electricity of the metal, and a compound of this kind obſerves thoſe laws conſiſtent with the phenomena.

Whence it is inferred that the diſengagement of hidrogen does not depend on a neg. ſtate; but that the electricity is communicated through the interpoſed liquid.

In each of the cells of the galvanic trough a quantity of this compound is diffuſed through the liquid. The moment a communication is made between the two ends, the electricity enters each of the copper-plates, leaving the hidrogen in bubbles

Probability that the hidrogen is in combination with electricity.

on

on the furface *. As this electrated hidrogen (if I may be allowed the expreffion) does not move through the liquid with the facility of the electricity itfelf, the neceffity of a furface of copper equal to that of the zinc is obvious. It is plain that only a proportionate quantity will be carried through the whole feries. This fact I have afcertained in attempting to render the apparatus propofed by Mr. Wilkinfon and yourfelf ufeful for all experiments. The fhock is even lefs when the furface of copper is lefs than a fquare inch.

Objection to the apparatus defcribed in our Vol. VIII. p. 3. on the couronne of Volta.

How the electricity increafes the hidrogen in the exterior cells.

The quantity of electrated hidrogen in each cell increafes from the copper end to that of the zinc in an arithmetical progreffion. The electricity only exifts in its fimple form while it is paffing from the copper furface through the two metals to the zinc furface. It there combines with another portion of hidrogen, which it leaves at the next copper furface, and fo on accumulating in quantity the longer the feries it has to pafs through. In the decompofition of water by the galvanic trough the veffels in which this is performed may be confidered as one cell in the feries. The electricity feizing the hidrogen at the pofitive wire and giving it out at the negative.

XI.

Memoir on the Origin of Wax †. *By* FRANCOIS HUBER, *Member of the Society of Natural Philofophy and Natural Hiftory of Geneva.*

IT has been thought ftrange that the word wax fhould feldom occur in a book which treats of bees alone: but neverthelefs as in the courfe of my obfervations, I had not attended to the products of their induftry, I could only have repeated what had been faid by Swammardam and Reaumur, and that did not feem to me to be neceffary.

Mechanical operations of the bees.

I knew that thefe infects collected abundantly upon the antheræ of flowers, that they are acquainted with the method of opening

* This fact is very obvious when the copper furface is very clean. The hidrogen under contrary circumftances is employed to reduce the oxide.

† From Journal de Phyfique, &c. Pluviofe, An. XII.

opening them, of gathering their duſt, keeping it in the cavities of their hind legs, and carrying it to their hives.

It had been obſerved that the particles of this duſt ſwells in water, and that, when one of them burſts, an oily liquor runs out, which floats on its ſurface, but did not mix with it; from theſe experiments, repeated on the duſt of a great number of flowers, it was concluded that they contain the principles of wax, but it was admitted that theſe muſt undergo a peculiar elaboration in the body of the bee, ſince, according to the experiments of Reaumur, a flexible wax could not be made from the duſt of the antheræ. Examination of the pollen of flowers;

It will be ſeen from ſeveral paſſages in my work that I had adopted this opinion: a ſingle obſervation of Burnens * changed all my ideas. The true origin of wax might have been ſooner known, had there been any ſuſpicion that it was not already diſcovered. I ſhall now ſtate how I was led to doubt, and what I have done to verify my new conjectures. The true origin of wax remained ſo long unknown from an opinion that it was diſcovered;

I was in Switzerland in 1793; the farmer of the eſtate on which I reſided had many bees, and the greater part of his hives having been ſtocked in former years, the combs with which they were filled reached to the ſtands, conſequently there was no room to conſtruct new ones. We remarked, however, that the working bees carried in a conſiderable quantity of this fecundating powder. Obſervations which led to the enquiry.

There was alſo in the ſame apiary ſome ſwarms of that year, the hives having only been ſtocked a day or two; in ſome of them the combs were only began, in others they were larger, but in all of them there were vacancies to fill up, and much work to do. We obſerved with aſtoniſhment that the bees of theſe ſwarms did not carry in the pollen, and that, nevertheleſs, they worked with activity in the conſtruction of new combs, and in lengthening thoſe already commenced. Where, therefore, did they procure materials for their edifices? After theſe obſervations, we ſuſpected that it was not from the duſt of the ſtamina, and that they had a very different uſe for it than that for which it was believed to be intended. We however found that it was not impoſſible to explain theſe extraordinary facts, without abandoning the hypotheſis of Reaumur,

* The name of M. Huber's ſecretary; which deſerves to be known to the cultivators of natural hiſtory.

mur, by ſuppoſing that the bees of the old hives ſtored up ſo much pollen in their combs for their future wants, while thoſe of the new ſwarms did not carry it outwardly on their legs, in the infancy of their eſtabliſhment, becauſe they had no cells in which they could depoſit it: it might be ſufficient to enable them to conſtruct their combs, if they were at liberty to fly to the flowers, procure their pollen, and return to their hives after having filled their ſtomachs, where it muſt be elaborated and converted into perfect wax. It was to obviate theſe doubts that I undertook the following experiments.

FIRST EXPERIMENT.

On Bees in Confinement with Honey alone for their Nouriſhment.

Exp. I. Is pollen neceſſary to the production of wax?

Muſt bees eat pollen to be in a ſtate to produce wax? This was the firſt queſtion which I thought it neceſſary to inveſtigate; the method of trying the experiment was obvious; it was only required to keep the bees within their hives, and thus prevent them from collecting or eating the fecundating powder.

On the 24th of May, Burnens lodged a ſwarm in a ſtraw hive, with as much honey and water as was neceſſary for their conſumption, and he cloſed the doors ſo that the bees could not get out and the air be at the ſame time renewed.

Wax from honey alone.

At firſt the bees were very uneaſy, but became calm on removing the hive to a cool dark place; their captivity laſted five days; they were permitted to come out in a room, the windows of which were ſhut: we then examined the hive more conveniently. We firſt noticed that there was no honey left in the veſſel which had been filled with it, with the ſole intention of feeding the confined bees; and were more aſtoniſhed to find five combs of the moſt beautiful wax, ſuſpended from the roof of the hive; they were perfectly white, and very brittle. This reſult was very remarkable; however, before forming a concluſion from it, that the honey with which theſe bees were fed had enabled them to produce the wax, it was neceſſary to enquire whether it could not alſo be explained in another manner.

The bees which I had employed had doubtleſs collected the duſt while they were at liberty.

They might have done ſo the evening before, or on the very day of their confinement, and might have enough in their ſtomachs, and in the cavities of their legs, to extract from it all the wax which we had found in their hive.

But

But if it was true that it had been obtained from the fecundating powder previouſly obtained, this ſource was not inexhauſtible, and the bees being unable to procure any more, they would ſoon ceaſe to conſtruct combs, and fall into the moſt complete inaction. It was neceſſary therefore to continue the ſame trial to render it deciſive.

The 28th, Burnens returned this ſwarm into its hive, after having taken out all the combs; he ſhut them up as before with a freſh ſupply of honey.

This experiment was not long, for on the evening of the ſecond day we perceived the priſoners working with new wax. The next day the hive was inſpected, and we found five combs, as heavy and as regular as thoſe made during their firſt captivity.

We afterwards repeated this experiment five times ſucceſſively, with the ſame bees, and the ſame precautions: we always found that the honey had diſappeared, and that new wax was produced. This reſult was ſo invariable during this long ſecluſion that we could no longer doubt that the honey alone had ſupplied them with all the elements of their wax, without the aſſiſtance of the fecundating duſt.

SECOND EXPERIMENT.

On a Hive from which Honey was excluded, and in which only Pollen and Fruits for the Nouriſhment of the Bees were left.

I thought it would not be uſeleſs to make the inverſe of the preceding experiments: it would ſhow me whether the pollen could not ſupply the want of honey when the bees were deprived of it, and enable them to produce wax. Exp. II.

I therefore encloſed a ſwarm in a bell-glaſs, in which had been placed a comb whoſe cells contained only pollen, and the ſole nouriſhment of the bees was fruit.

Theſe bees did not make wax, nor did they form a ſingle cell during eight days, which was the time of their captivity.

I was going to repeat this experiment, when Burnens remarked that the free bees were, in ſome meaſure, in the ſame ſtate as thoſe we had confined; there being no honey at that time in the flowers, they found only pollen, and did not work in wax. Bees fed on pollen and fruits do not produce wax.

I may

I may perhaps be aſked how I was ſatisfied of this, to which I anſwer, bees-wax is white at firſt, the cells ſoon become yellow, and in time, this colour grows browner, and in older hives the combs have acquired a blackiſh tinge. It is therefore very eaſy to diſtinguiſh the new cells from thoſe which have been ſome time formed, and conſequently to know whether the bees are really making combs, or whether that work is ſuſpended; it is ſufficient to raiſe the hives, and to notice the lower edges of the combs.

The odour exhaled by the hives, and the ſhape of the bees, are indications by which it may always be known whether there is honey in the flowers; if they are combined, there can be no further doubt, and, particularly, if a great number of bees return to the hive, which are remarkable for the bulk and the form of their bellies. Thoſe which are filled with honey have the abdomen cylindrical; the name of wax-making bees belongs to them excluſively: the bellies of the labouring bees which have other functions, always preſerve their ovoid form, and their volume is never ſenſibly augmented; the name of nurſing bees is proper for theſe.

Wax-making bees.

Nurſing bees.

The farmers of the neighbouring villages kept their bees in baſkets, or in caſes of different forms; and I was able to viſit a very great number without going to any great diſtance from my habitation.

Operations of the bees during an intemperate ſpring,

In 1793, an intemperate ſpring had retarded the ſeparation of the ſwarms; there had not been any in the country before the 24th of May; but towards the middle of June there were ſeveral in the vicinity of my reſidence. At that time the fields were covered with flowers, the bees collected much honey, and the new ſwarms worked at the wax with vigour.

On the 18th, Burnens viſited ſixty-five hives; at the entrances of all of them he obſerved wax-making bees. Thoſe which returned to old hives, not having to conſtruct cells, depoſited their honey in the combs, or diſtributed it among their companions; thoſe belonging to the ſwarms converted their honey into wax, and haſtened to conſtruct combs for the reception of their young bees.

It was ſhowery on the 19th: the bees went abroad but brought home only pollen. The weather was cold and rainy until the 27th. We were deſirous of knowing if this had prevented their working. On the 28th, all the hives were lifted:

Burnens

Burners found that the work had been ftopped; the combs which he had meafured on the 19th, were not at all increafed, and were of a citron-yellow, nor was there a fingle white cell in any of thefe hives.

On the 1ft of July the chefnuts and limes were in bloffom, the thermometer indicated the 20th degree; the wax-making bees re-appeared, they carried away great quantities of honey, which, as we had before obferved, was employed in augmenting the provifions of the old hives, and in enabling the young fwarms to conftruct new combs. —and a dry fummer.

The greateft activity was obfervable among them: the gathering of honey, and the production of wax continued until the middle of this month.

July 16th, the heat remained the fame: the field flowers, as well as thofe of the chefnuts and limes, were completely withered; they yielded no more honey; their pollen alone attracted the working bees, and they collected it abundantly, but there was not any wax produced; the combs were not lengthened; thofe of the young fwarms did not fill more than two thirds of their hives.

Auguft 9th. It had not rained for fix weeks, the heat was very powerful, nor was there any dew to allay it during the night: the black wheat which had been in flower for fome days, did not offer any honey to the bees; they found only pollen.

On the 10th, it rained for feveral hours; next day the black wheat had the odour of honey; in fact it might be feen glittering in their expanfed flowers. The bees found enough to feed them, but too little to induce them to work at new wax.

On the 14th, the drought re-commenced, and lafted to the end of the month: no more honey appeared upon the flowers, and when we vifited the fixty-five hives for the laft time, we found, 1ft, that the bees had not produced any wax after the middle of July; 2d, that they had ftored up a great quantity of pollen; 3d, that the fupply of honey was much leffened in the old hives, and that hardly any remained in the new fwarms, that which they had collected in the fpring having been employed in the preparation of wax; the pollen, therefore, has not this property, and no further doubt remained on this head.

This year had not been ftormy, and I have fince afcertained, by a great number of obfervations, that electricity is fingularly favourable Weather moft favourable to the labour of bees.

favourable to the ſecretion of honey by the flowers: the bees never collect it in greater abundance, nor is the preparation of wax ever more active than when the wind is in the ſouth, the air humid and warm, and a ſtorm gathering.

Heat too long continued, and the drought which is the conſequence of it, cold rains, and principally a north wind, ſuſpend it entirely.

THIRD EXPERIMENT.

On the Uſe which the Bees make of the fecundating Powder.

Exp. III. In the ſecond experiment the bees did not touch the pollen which I had placed within their reach, and, as its quantity was not ſenſibly diminiſhed during this trial, I was induced to believe that it was not an aliment proper for them.

What is the uſe of pollen? I alſo knew that the new ſwarms were liable to periſh from hunger in the middle of ſummer, and even when the country was covered with flowers, if a particular temperature, which is too uncommon in our climate, did not favour the ſecretion of honey in their nectaria. What, therefore, is the uſe of the pollen which they collect with ſuch avidity during eight months of the year, and of which they lay up ſuch abundance?*

This queſtion remained to be inveſtigated.

I had a hive, in diviſions, the queen of which was barren; its combs did not contain any pollen, but they had much honey; the two narroweſt ſides of this hive were formed of panes of glaſs, through which the ſurfaces of the exterior combs might be ſeen, and the conduct of the bees obſerved.

The queen, bee taken away. I took away the queen on the 16th of July, but to conſole the working bees I removed the firſt and the twelfth combs, in which there was not any thing to intereſt them, and I ſupplied their places with two combs, the cells of which were filled with eggs, and worms of all ages. I carefully cut away all the cells in which pollen could be perceived, and ſhut up the

* Reaumur was of opinion that the bees of a well-ſtocked hive might collect at leaſt a hundred pounds of this ſubſtance in the courſe of a year; but, having remarked that the weight of wax, fabricated in the ſame time, did not exceed two pounds, he concluded "that the bees extract only a very ſmall portion of the true wax from this native wax, that the greateſt part of it is required for their nouriſhment, and, that the reſt is diſcharged from their bodies in the form of excrement."

hive

hive with a grating. My intention will be gueffed: I wifhed to know whether thefe infects could fupport their young without this fecundating powder.

The next day nothing extraordinary occurred; the bees fat on their eggs and feemed to nurfe them.

On the 18th, after fun-fet, a great noife was heard in the hive. Anxious to fee what occafioned it, we opened the fhutters, and obferved that all was in confufion: the incubation was ftopped; the bees ran over the combs in diforder; we faw thoufands precipitate themfelves on the ftand, thofe which were neareft to the mouth eagerly gnawed the grating; their intention was no longer doubtful, they wifhed to get out of their confinement.

I was fearful of deftroying them by continuing to prevent them from yielding to their inftinct, they were therefore fet at liberty: the whole fwarm came out, but the hour was unfavourable to their collecting, the bees did not go far from the hives, the darknefs, and the chillinefs of the air, foon compelled them to return, and probably calmed their agitation, for we faw them quietly reafcend their combs, and order appeared to us to be re-eftablifhed. This moment was taken to clofe the hive again.

On the 19th, we faw two royal cells begun on one of the combs of the nurfery *(couvain)*; the evening of this day, and at the fame hour as the day before, we heard a great tumult in the clofed hive, it was in a general confufion, and we were again obliged to permit the fwarm to come out.

Other royal cells made,

The 20th was the fifth day of their captivity; we thought it had been of fufficient duration, and were alfo very impatient to examine the nurfery, and to fee what was the caufe of the periodical agitation of thefe bees: Burnens therefore opened the firft and twelfth windows, and drove the bees from the combs, fuffering them to take their flight in a room, the windows of which were fhut.

He firft noticed that the royal cells had not been continued, that they did not contain any worm, and that there was not an atom of the jelly which ferves for the nourifhment and the cradle of the larvæ of the queens.

He fought in vain for eggs, for worms, and for the liquid in the common cells; all had difappeared. Had thefe worms died

—but the procefs did not go on without pollen.

died of hunger? Had we, by withdrawing the fecundating powder, deprived the bees of every means of nourishing them?

To ascertain this it would be sufficient to restore them their pollen, and observe the issue. The bees were therefore again returned to their prison, after having substituted young worms for those which had been suffered to die.

On the 22d, we found that the bees had fastened these combs, and that they were again in a state of incubation; we then gave them some pieces of combs in which other bees had stored up the fecundating powder, and, the better to observe what they did with it, we took some of the pollen out of the cells, and laid it exposed on the stand of the hive. In a few minutes the bees discovered the pollen in the combs, and that which we had taken out; they took it grain by grain in their jaws, and conveyed it into their mouths; those which had eaten most voraciously re-ascended the combs, and placed themselves, at first, upon the cells of the young worms, which they entered with their heads foremost, and remained there a greater or less length of time. One of the windows of the hive was now opened cautiously, Burnens powdered the bees which eat the pollen, and watched them for some hours; he observed that the marked bees always re-ascended to the nursery, and immediately entered the cells of the young bees.

The 23d, we found the royal cells begun.

The 24th, we drove the bees from off the young worms, and we remarked,

1st, That all of them had the jelly, as in the common hives:

2d, That the worms had grown larger, and were forwarder in their cells:

3d, That others had been shut up again: And,

4th, That the royal cells had been lengthened.

The 25th, we withdrew the pieces of comb which we had placed on the stand, and found that the quantity of pollen was sensibly diminished; we afterwards replaced them in the hive with other cells filled with the fecundating powder.

The 26th, the royal cells had been closed during the night, as well as several of the common ones.

The 27th, I restored these bees to liberty; Burnens examined the cells with the greatest attention, and found jelly in all those which

which ſtill contained worms, but moſt of them were ſhut with a lid of wax: he examined ſome of the latter, and found the worms employed in ſpinning cocoons of ſilk.

All the worms had therefore been tended as in the natural hives. In this ſecond trial we did not perceive any diſorder in this hive; there had not been the leaſt agitation: it is true ſome of the working bees attempted to go out in the courſe of the day, but finding it impoſſible, they re-aſcended the combs quietly, which were never left for an inſtant. The hive being abundantly ſupplied with honey, and with the pollen neceſſary for their young, left them nothing to wiſh for; and they were ſtill more happy when a queen was born, who afterwards became pregnant, and laid a great number of eggs.

After theſe two experiments there could be no more doubt that the fecundating duſt was the aliment proper for the young bees, and that the want of this ſubſtance was the cauſe of their death, and of the evident anguiſh of their nurſes during their firſt captivity.

The pollen is therefore the food of young bees.

FOURTH EXPERIMENT.

On Bees deprived of Honey and Pollen, and which it was attempted to feed with Sugar.

I wiſhed to know if it was the ſaccharine part of the honey which enabled the bees to produce wax.

Exp. IV. What part of the honey contains the principles of the wax?

Burnens confined a ſwarm in a glazed hive: one pound of Canary ſugar was their ſole aliment.

He put a ſecond ſwarm into another hive, and endeavoured to feed them with very coarſe raw ſugar; and to obtain a term of compariſon, a third ſwarm was ſhut up in the ſame manner, and fed with honey.

The bees of the three hives produced wax; thoſe fed with the different qualities of ſugar produced it ſooner than the ſwarm which had only had honey, and they produced it in greater quantity.

A pound of Canary ſugar reduced to ſyrup, and clarified with white of egg, yielded 10 gros, 52 grains, of a wax not ſo white as that which the bees extract from honey.

Wax made from true ſugars of different qualities.

An equal weight of raw ſugar gave 22 gros of very white wax.

Maple

Maple ſugar produced the ſame effect. This experiment having been repeated ſeven times ſucceſſively, always employing the ſame bees, we could not doubt that ſugar contains the principles of wax, and we concluded that it was the ſaccharine part of the honey which had this property.

CONCLUSION.

Concluſions. These obſervations ſhew,

1ſt, That the wax comes from the honey:

2d, That the honey is alſo a food of the firſt neceſſity to the bees:

3d, That flowers do not always contain honey, as has been imagined; that this ſecretion is ſubject to the variations of the atmoſphere; and, that the days in which it is abundant are very rare in our climate:

4th, That it is the ſaccharine part of the honey which enables the bees to produce wax:

5th, That raw ſugar yields more wax than honey, or refined ſugar:

6th, That the duſt of the ſtamina does not contain the principles of wax:

7th, That this duſt is not the food of the adult bees, and that they do not collect it for themſelves:

8th, That the pollen affords the only aliment which is proper for their young, but that this ſubſtance muſt undergo a peculiar elaboration in the ſtomachs of the bees to be converted into an aliment, which is always appropriated to their ſex, their age, and their wants, ſince the beſt microſcopes do not ſhew the particles of pollen, or their coverings in the liquor prepared by the working bees.

I ſhall ſpeak of the economical conſequences of theſe obſervations on another occaſion. By ſhowing the breeders the real wants of the bees, they will be poſſeſſed of the means of aſſiſting them in time, in all their neceſſities, and of preſerving them in climates in which nature has not placed them, and in which they could not proſper without our aid.

An

XII.

An Enquiry concerning the Nature of Heat, and the Modes of its Communication. By BENJAMIN *Count of* RUMFORD, *V. P. R. S. &c. Abridged from the Philosophical Transactions for* 1804.

(Concluded from p. 63.)

Exp. 12. TWO equal cylindrical veſſels of ſheet braſs poliſhed very bright, each three inches in diameter and four inches long, ſuſpended by their oblique necks in a horizontal poſition, being placed on their wooden ſtands, were filled with water at the temperature of 180°; and their circular flat bottoms were preſented, in a vertical poſition, to the two balls of the thermoſcope, at the diſtance of two inches.

Exp. 12. Bodies alike in all reſpects and heated, do, at equal diſtances, affect the thermoſcope equally.

When the two hot bodies were preſented, at the ſame moment, to the two balls of the inſtrument, or what was ſtill better, when two ſcreens were placed before the two balls, at the diſtance of about an inch, and, after the hot bodies were placed, theſe ſcreens were both removed at the ſame inſtant, the bubble remained without motion in the middle of the horizontal part of the tube of the inſtrument.

If the diſtances of the equally hot bodies were rendered unequal, the bubble always moved towards the moſt remote of the two; and if a ſingle hot body was preſented to one of the balls, the bubble was driven from it, and might have been carried quite out of the tube; which, however, was always avoided, as the inſtrument would have been by that means quite deranged.

Exp. 13. The flat circular bottom of one of the cylindrical veſſels was blackened by holding it over the flame of a wax candle, the other veſſel remaining bright as before. Both were then filled with water at 180°, and preſented at equal diſtances to the two oppoſite balls of the inſtrument, as deſcribed in the laſt experiment.

Exp. 13. A cylinder blackened with ſmoke, gives out more radiant heat than a clear metallic ſurface.

The bubble was inſtantly driven out of its place by the ſuperior action of the blackened ſurface; and it did not return to its former ſtation until the blackened ſurface had been removed to more than eight inches from the ball to which it was preſented;

prefented; the other, which had not been blackened, remaining in its firft pofition, at the diftance of two inches.

The other coatings which had accelerated the cooling, were alfo found to increafe the radiation.

Other fimilar experiments were made to fhew whether thofe coatings or clothings which, in the former experiments with the large veffels, had accelerated their cooling, did alfo increafe their power of radiation, as fhewn by the procefs and inftruments laft defcribed. And the refults invariably proved that this is the cafe.

It was alfo a queftion deferving to be inveftigated, whether any peculiarity belonging to the metallic furface hitherto ufed, namely of brafs, might have influenced the refults.

Exp. 14, 15. All metallic furfaces radiate equally.

Exp. 14, 15. The two large cylindrical veffels No. 1 and 2, were covered with a fingle coating of oil varnifh, and on this, when fufficiently dry, was laid a covering of thick gold leaf upon No. 1, and thick filver leaf on No. 2. Thefe veffels were cooled through the interval of 10 degrees, in the fame time as the naked veffel ufed as the ftandard. Similar experiments with veffels of tinned iron and of lead, fhewed that the radiation from all thefe metals, though fo different in their conducting power, is the fame.

" Is not this," fays the Count, " owing to their being all equally wanting in tranfparency? And does not this afford us a ftrong prefumption that heat is, in all cafes, excited and communicated by means of radiations or undulations, as I fhould rather choofe to call them?"

And he proceeds to obferve, that another very important queftion alfo muft be decided before thefe points can be determined, and that is, Whether bodies are cooled in confequence of the rays they emit, or by thofe they receive? The celebrated experiment of Pictet has fhewn, in our author's opinion, that rays or emanations *proceed from cold bodies,* which may be concentrated by concave mirrors, and will affect a delicate air thermometer.

Exp. 16, 17. A cold body depreffes the temperature of other bodies by radiation. Metal horizontally placed.

Exp. 16, 17. The horizontal cylindrical veffels (*Fig.* 3, *Plate I.* of the prefent volume) were made very clean and bright, were duly fixed, and left for feveral hours in a room near the thermofcope; and when each veffel was in fucceffion prefented to that inftrument, with every precaution to prevent irregularity from external circumftances, it was not found to be affected by them. One of the veffels was now taken away and filled with ice and water, and then prefented, at the diftance

diſtance of four inches, to one of the balls of the thermoſcope. The bubble immediately moved towards the cold body, and paſſed through the ſpace of one inch. A nearer approach of the cold body produced a ſtill farther motion in the ſame direction.

Exp. 18, 19. Though this reſult appeared to the Count to prove indiſputably that cold bodies emit rays capable of generating cold in warmer bodies, yet, from the importance of the fact, he choſe to vary the ſubſtance preſented to the inſtrument, as well as to remove all ſuſpicion of the action of cold currents of air. He therefore laid the thermoſcope on one ſide, and placed underneath one of its balls a ſolid cake of ice, at the diſtance of ſix inches. The reſult of this experiment was the ſame as of the other, and the bubble was moved one inch in the tube. Ice-cold water produced the ſame effect as ice itſelf.

Exp. 18, 19. Ice and ice-cold water underneath the thermoſcope, cooled it by radiation. No current of air could here *aſcend.*

Exp. 20. Whether this effect of cold bodies be governed by the ſame laws as thoſe obſerved by varying the nature of the ſurface in heated bodies, or by any other, now remained to be aſcertained. It was before found that metal blackened over a candle, did emit much more of calorific rays than the ſame metal when naked. The ſame experiment was now made with cold bodies. One of the cylinders had its end blackened, and the other cylinder was left bright and naked. Both were filled with ice and ſalt, and at the ſame inſtant they were ſuffered to act from equal diſtances on the thermoſcope. The bubble moved towards the blackened body; not indeed ſo much as when the bodies were heated in the former experiments, becauſe the temperature was not here ſo far diſtant from the common temperature of ſurrounding bodies as in that experiment; but on ſeveral repetitions of the experiment with theſe cold bodies, the effect was conſtantly the ſame. It was found that the precipitation of ice out of the ſurrounding air tended ſpeedily to raiſe the temperature, and alſo that the clean ſurface, when covered with ice, had a greater frigorific effect than when the metal was naked.

Exp. 20. Metal blackened with ſmoke cooled the inſtrument more, by radiation, than a clear ſurface at ſame temperature.

Ice radiates more than metal.

Exp. 21. The radiation of heat from animal ſubſtances appearing, from ſome facts, to be conſiderable, a piece of goldbeater's ſkin was applied wet to one of the veſſels, and remained firmly adherent when dry. This, when filled with hot water at 180°, and preſented to the thermoſcope in oppoſition

Exp. 21. Goldbeater's ſkin radiates 25 times more than metal. Hot water in the veſſels.

to the other veſſel clean and bright, and alſo filled with the ſame fluid at the ſame heat, was found to be much more calorific. The bubble which moved from the coated body, did not return to its ſtation until this body was removed to a diſtance five times as great as that of the other. Whence our author concludes, that it emitted twenty-five times the quantity of calorific rays.

Exp. 22. Similar effect with ice-cold water. The gold-beater's ſkin produced more cold by radiation.

Exp. 22. The veſſels uſed in the laſt experiment were emptied and refilled with ice and water. They were then preſented at equal diſtances from the reſpective balls; and the effect of the body which was covered with gold-beater's ſkin, was much more conſiderable in producing cold.

The radiation of cold bodies appearing to the Count thus to be proved beyond all doubt, he was deſirous of aſcertaining whether the frigorific rays poſſeſs an equal power of affecting the temperature of bodies as the calorific rays do; the temperatures of the radiant bodies being at the ſame diſtance each way from the body to be acted upon.

Exp. 23. A hot and a cold veſſel equally diſtant from the common temperature, affect the thermoſcope equally.

Exp. 23. With this intention, one of the veſſels filled with pounded ice and water, was preſented to a ball of the inſtrument, and the other veſſel filled with water at 112°, was preſented at an equal diſtance on the oppoſite ſide of the ſame ball; the temperature of the room and inſtrument being 72°, or 40° diſtant from each of the temperatures of the veſſels, and the other ball of the inſtrument being defended from all radiation by ſcreens. The bubble remained motionleſs; ſo that the oppoſite actions were in fact equal. And when either veſſel was drawn farther off, the effect of that veſſel became leſs, and the bubble moved; that is to ſay, towards the ball if the cold veſſel were neareſt, or from the ball if the hotter; and theſe effects were equal in quantity as well as in celerity of motion.

Why the cooling by radiation has been leſs noticed.

The Count again repeats his concluſion from theſe experiments lately exhibited to Profeſſor Pictet, M. de Sauſſure, and M. Senebier at Geneva, that the rays which generate cold are juſt as real and juſt as intenſe as thoſe which generate heat; and he proceeds to account for this reſult having been overlooked, by obſerving, that the degrees of cold we are able to produce, are much leſs diſtant from the uſual temperature than thoſe of heat, which are within our power. Thus a cannon ball, heated to 160 degrees, or 70 degrees above blood-

heat,

heat, would radiate quite as much as ice; and a bullet of freezing mercury would radiate ſcarcely more than another of boiling water: both which hot temperatures are very trifling in compariſon to the heat in which we are habituated to notice and obſerve this phenomenon.

Exp. 24. After the proof that cold bodies of the ſame kind affect the thermoſcope equally, when equally diſtant from the common temperature, it remained to be determined whether the different modifications of ſurface have the ſame effects in the ready propagation of lower temperatures, as they had ſhewn before in higher. To ſhew this, it was only required to oppoſe them to each other, as to their action upon the ſame ball, as in the laſt experiment. With this view both diſcs were blackened, and, the temperature of the room being 72° as before, one of them was charged with ice and water, and the other with water at 112°. Theſe, at equal diſtances from the ball as before, did not affect the bubble, and therefore their actions were preciſely equal.

Exp. 24. The ſame ſurfaces which produce moſt heat by radiation, do alſo produce moſt cold.

In the conſideration of two kinds of rays, calorific and frigorific, it did not eſcape the attention of our author, that hot and cold are terms denoting mere relations; ſo that the ſame body will be either hot or cold accordingly as the common temperature, or temperature of the bodies of compariſon, is lower or higher. Queſtioning, therefore, as to the difference between calorific and frigorific rays, he demands whether the ſame rays may not be either calorific or frigorific, accordingly as the body at whoſe ſurface they arrive, is hotter or colder than that from which they proceed?

Do not the ſame rays produce heat or cold, as the temperature of the emanating bodies is higher or lower than the receiving body.

Exp. 25. The whole of the external ſurface of one of the large cylindrical paſſage thermometers, *Fig.* 1, *Plate I.* was covered with gold-beater's ſkin, and, along with the ſtandard inſtrument of the ſame kind, was filled with hot water. The covered inſtrument cooled through the ſtandard interval of ten degrees, namely, from $101\frac{1}{2}$ to $91\frac{1}{2}$, in twenty-ſeven and three quarter minutes; but the naked inſtrument employed 45 minutes in paſſing through the ſame interval.

Exp. 25. Metal covered with gold-beater's ſkin cools faſter.

Exp. 26. Both inſtruments were ſuffered to remain in the cold all night, when the temperature in the naked inſtrument was $50\frac{1}{8}$°, and that of the covered inſtrument $49\frac{1}{4}$°; the air of the room being 48°. Both inſtruments were then removed into a warm room, of which the temperature continued be-

Exp. 26. And alſo acquires heat more quickly.

tween

tween 64° and 65°. The covered inftrument acquired heat the moft rapidly; for in a quarter of an hour both ftood at $51\frac{1}{2}°$, and at the end of four hours the naked inftrument fhewed the temperature 61°, and the covered $63\frac{1}{2}°$: whence the Count obferves, that thofe fubftances which part with heat the moft readily, have alfo the greateft facility to acquire it.

Rays received as well as thofe emitted, alter the heats of bodies.

He then proceeds to reafon on the probability that the temperatures of bodies may be changed, not only by the rays they emit, but by thofe they receive from other bodies; and as the cooling of hot bodies is fo much accelerated by covering their furfaces with fubftances which affect the radiation or abforption, he thinks it highly probable that the air is but little affected as to its temperature, by thefe rays which pafs through it; and he contemplates the eftablifhment of this fuppofition, as promifing to explain various interefting phenomena; particularly that of the fteady temperature of living animals, notwithftanding the great quantity of heat generated in their lungs, and the different temperatures of the fluid which furrounds them.

Radiation does not heat the air.

Steady temperature of animals,

may be affected by radiation.

For it is evident, the greater power an animal may poffefs of throwing off heat by radiation, independently of the effect of the contact of the furrounding air, the lefs will his temperature be affected by the changes in that fluid, or the oppreffive heat of the climate. The probability that negroes and people of colour, who fupport the heats of tropical climates much better than white people, offers itfelf in this place as the confequence of their colour; a quality which not only enables them to throw off heat, but even, as the Count much fufpects, to abforb frigorific rays from fuch bodies as may emit them.

Inftance in negroes.

Exp. 27. Black paint upon animal membrane increafes radiation. Emiffion,

Exp. 27. When the flat ends of both the horizontal cylindrical veffels were covered with gold-beater's fkin, and one of them painted black upon the covering with Indian ink, this laft, as indicated by the thermofcope, emitted more calorific rays from the included hot water than the other veffel did.

and alfo reception.

Exp. 28. When the fame two veffels filled with boiling-hot water, were fet to cool, the blackened veffel cooled through the ftandard interval of ten degrees in $23\frac{1}{2}$ minutes, while the other, which was not blackened, employed 28 minutes.

4 The

The author again reverts to the application of theſe facts to animal bodies. Whether the oily coating which ſavages apply to their ſkins in cold climates, may not add to their comfort by reflecting frigorific rays; whether the Hottentots, ſtill more diſguſtingly beſmeared, may not derive advantages ſimilar to thoſe derived by negroes from their black colour? are queſtions that promiſe to lead to reſults of practical value. He then proceeds to explain more fully the manner in which negroes may be ſuppoſed to reſiſt the action of a burning ſun. An oil exudes from the ſkin of theſe people when expoſed naked to the ſun; which oil reflects the ſun's calorific rays. Heat more intenſe produces ſweat, which not only aids the former proceſs, but generates cold by evaporation. But when the ſun is ſet, the oil retires from the ſurface, and the ſkin becomes well adapted to admit frigorific rays from the neighbouring bodies.

Effects of the oil on the ſkins of Hottentots, &c. and of the natural changes in the ſkins of negroes.

Exp. 29. The thermoſcope was perceptibly affected by the radiation of cold bodies: It was deſirable to know whether this effect would be ſhewn in a groſſer way, by accelerating the cooling of a hot body. For this purpoſe, two conical veſſels of thin ſheet braſs, each four inches diameter at the baſe, and four inches high, ending above in a cylindrical neck, were ſeparately encloſed in a cylinder of thin paſteboard covered with gilt paper, and then the veſſels were covered up with rabbit-ſkins having the hair on them, in ſuch a manner that no part of theſe veſſels, except their flat bottoms, was expoſed naked to the air. The bottoms were covered with gold-beater's ſkin painted black with Indian ink, in order to render them as ſenſible as poſſible to calorific and frigorific rays.

Exp. 29. Veſſel of water more rapidly cooled by radiation from ice.

The two veſſels thus prepared were ſuſpended, with their bottoms downwards from the arms of a ſtand, and under each was placed a pewter platter blackened on the inſide by ſmoke from a candle. The platters themſelves were ſupported on ſhallow earthen diſhes which reſted upon wooden ſtands; each pewter platter having a perforated cover of thick paper, in the center of which was a hole ſix inches in diameter. The diſtance from the floor of the room to the ſmoked ſurface of each platter was 40 inches, and the interval between the ſurface of each conical veſſel and its correſpondent platter immediately beneath, was four inches. One of the platters remained at the tempera-

ture

ture of the room, but the other was kept conſtantly ice cold by means of pounded ice and water contained in the diſh beneath it.

The two conical veſſels were now filled with boiling hot water, and every precaution was taken to prevent agitation in the air; the only means by which an aſcent of cold fluid could be ſuſpected. The reſult of the experiment was, that the veſſel ſuſpended over the ice-cold platter cooled from 50° to that of 40° above the temperature of the room, in 33 minutes and 42 ſeconds; whereas the other veſſel required 39 minutes and 15 ſeconds to cool through the ſame interval.

Exp. 30. Repetition.

Exp. 30. Upon repeating this experiment the next day, the times were 33 minutes 15 ſeconds and 39 minutes 30 ſeconds.

Velocity of cooling accelerated by the radiation from ice, in the ratio of 5 to 4.

As the cooling of theſe veſſels is a complicated proceſs which includes the conſideration of the heat that paſſed through the covered ſides, in theſe different times; the Count enters into a proceſs of computation, founded on the principles made uſe of at page 61, by which he determines that the velocity with which the heat paſſed through the bottom of the veſſel expoſed to the ice, was to that with which it paſſed through the bottom of the other veſſel as five to four nearly.

Exp. 31. Proceſs to ſhew the heat abſorbed by the air when a current was allowed to take place. It was then one twenty-ſeventh of the whole loſs.

Exp. 31. By the manner of defending the two conical veſſels each had a circular band or hoop of the fine poſt paper which projected half an inch below its baſe. It is evident that the air in this ſpace could not paſs upwards, as it became heated by contact, and conſequently, that very little of the cooling effect could have been produced by the contact of an aſcending current. To aſcertain what this effect might have been, the two veſſels were ſuſpended as before; but one of them had its baſe inclined in an angle of 45°, while the other baſe continued horizontal. In this ſituation they were filled with boiling water, and ſuffered to cool without the platter and ſtands beneath them. Two effects would follow from theſe arrangements: the veſſel, which in the other experiment had been placed over a platter at the temperature of the room, would now cool a very little faſter by the abſence of that platter, which no doubt muſt have had its temperature a little raiſed; and the inclined veſſel in the preſent experiment would be cooled ſomewhat more ſpeedily, by the ſucceſſive contact of the aſcending current of air which was at liberty to riſe. This veſſel was found to cool through the ſtandard interval of 10° in 37¼ minutes, and the horizontal

horizontal veſſel employed $38\frac{1}{2}$ minutes. By referring theſe facts to computation, the Count finds that the heat loſt by actual communication to the air, is nearly $\frac{1}{27}$ part of the whole loſs.

Exp. 32. The counter radiation by the platter, which was ſtated under the laſt experiment as impeding the cooling proceſs, affords the important proſpect of explaining the effect of clothing, and therefore deſerved to be more fully examined. The experiments 29 and 30 were therefore repeated, with the diſtance of three inches only between the bottom of each veſſel and its correſponding platter. The times of cooling through 10°, were now $33\frac{1}{4}$ minutes and $40\frac{1}{4}$ minutes.

Exp. 32. Velocity of cooling retarded by the vicinity of other bodies. Hence clothing.

Exp. 33. And when the diſtance was diminiſhed to two inches, the times proved $32\frac{1}{2}$ minutes and $42\frac{3}{4}$ minutes.

Exp. 33. Repetition.

Theſe experiments ſhew that the vicinity of a cold body, of which the low temperature is not kept up by artificial means, retards the cooling of a hot body. And from this fact the Count concludes, that if the hot body had been a globe ſuſpended in the center of another larger thin hollow ſphere, of the ſame temperature, at the commencement, as the air and walls of the room, the cooling would have been more ſlow than if the external globe had not been preſent; and alſo, if the external globe itſelf were included in another globe of the ſame deſcription, the retardation would have been ſtill more conſiderable. And by extending this ſuppoſed experiment to a number of thin concentric hollow ſpheres, we may conceive a great retardation to follow, and ſhall become acquainted with the nature of the effects which take place when a hot body is ſurrounded by proper clothing.

The vicinity of many thin bodies in ſucceſſion will conſtitute a clothing;

If the ſpheres were metallic, the cooling would be ſlower if the ſurfaces were poliſhed than if unpoliſhed or blackened; whence it is highly probable, that the warmth of any clothing depends very much upon the poliſh of its ſurface.

more effectual if the ſurfaces be poliſhed.

The microſcope ſhews that thoſe ſubſtances which ſupply us with the warmeſt coverings, ſuch as furs, feathers, ſilks, and the like, have their ſurfaces highly poliſhed; and the finer the fibres, or the greater number of interpoſed poliſhed ſurfaces, the warmer is the clothing.

Furs, feathers, ſilks, &c. are highly poliſhed.

In the former experiments of Count Rumford, he conſidered the warmth of clothing as principally depending on the obſtacles it oppoſes to the motion of the ſurrounding cold air; but

Clothing defends much more by radiation than by preventing circulation of the air.

but, by a patient and careful examination of the ſubject, he is convinced that the efficacy of radiation is much greater than he had ſuppoſed: from the reſult of the experiment No. 31, it appears that a very ſmall part of the heat of a body cooled in the air, is in fact communicated to that fluid; much the greateſt portion being communicated to ſurrounding bodies at a diſtance; and, in one of his former experiments, a hot body was cooled, though it was placed in a torricellian vacuum.

Air heated only at its ſurface and by radiation.

He conſiders the heat which air receives by coming in contact with a hot body, to be communicated by radiation, in the ſame manner as it is received by other bodies at a greater diſtance; and he apprehends that the contiguous particle receives the heat in preference for no other reaſon than becauſe it is at the ſurface of the fluid, this being the place where reflection, refraction, and increaſe of temperature, take place; and, from theſe conſiderations, he explains what has been called the non-conducting power of tranſparent fluids.

Analogy between light and heat.

By extending the analogy of thoſe facts which we know concerning the effect of poliſhed ſurfaces on light, to the radiations of heat alſo, the preceding facts are eaſily explained. The frigorific rays are reflected externally, and a large portion of the calorific rays, which would have iſſued forth through a rough ſurface, are, in the other caſe, turned inwards by reflection.

A drop of water reſiſts the heat of ignited iron, by its poliſh.

The poliſhed ſurface of a drop of water, which rolls about at a diſtance from the face of a piece of red-hot iron, enables it to reflect the calorific rays; the water acquires little heat, and evaporates ſlowly.

With a leſs heat the water enters the pores of the oxide upon the metal, loſes its poliſh, acquires heat very rapidly, and is ſoon evaporated.

It retains its poliſh longer in ſilver, and reſiſts more.

If the metal be leſs oxidable, as, for example, a ſilver-ſpoon, the drop of water will ſupport or reſiſt a lower heat. In fact it does not ſo ſoon loſe its poliſh; but at a ſtill lower heat, that is to ſay, a little above boiling-water, a drop of water is inſtantaneouſly evaporated.

Exp. 34. Water rolling in a ſpoon blackened with ſmoke, cannot be made to boil.

Exp. 34. A clean poliſhed ſpoon, rendered black by holding it over the flame of a wax candle, will receive a large drop of water, which will roll about without wetting the blackened ſurface. This drop cannot be made to boil by holding the ſpoon over the flame of a candle. When the

ſpoon

ſpoon is too hot to be held by its handle, the drop of water poured into the palm of the hand is warm, but by no means ſcalding hot.

Exp. 35. If a large drop of water be formed at the end of a ſmall ſplinter of light wood, and the drop be thruſt quickly into the center of the flame of a newly ſnuffed candle, it will remain for a conſiderable time in the center of the flame, without being apparently affected by the heat; and if it be taken out of the flame and put upon the hand, it will not be found to be ſcalding hot. If it be held for ſome time in the flame, it will be gradually diminiſhed by evaporation; but it does not appear that the heat is communicated by the flame, but by the wood to which it adheres, which is ſoon heated, and at laſt ſet on fire.

Exp. 35. Drop of water put into the flame of a candle, is not heated.

The remainder of the Count's memoir conſiſts of theoretical remarks and inferences, occupying 29 pages of the Tranſactions. I have endeavoured faithfully to deſcribe the facts in the way of abridgement, but cannot with the ſame facility do juſtice to theſe argumentative reſults. I ſhall therefore, for the preſent, conclude my account of his paper; but may not perhaps wholly overlook his theories upon ſome future occaſion.

Why the theoretical remarks are not here abridged.

XIII.

Letter from Profeſſor VEAU-DE-LAUNAY *to* J. C. DELAMETHERIE, *on fulminating Silver**.

AS it is at all times uſeful to ſtate facts, whatever may be the reſults, I think it right to inform you of an accident which occurred in my laboratory.

Accident with fulminating ſilver.

I had employed one of my pupils, a very good operator, to prepare a ſmall quantity of fulminating ſilver, which he executed with ſkill.

The quantity obtained was about five grains, or a quarter of a gramme: it was depoſited in a cryſtal capſule about two lines in thickneſs. He had taken a ſmall quantity, about half a grain, which was ſeparated with a card, and then dried, and

* From Journal de Phyſique, &c. Floreal, An. XII.

afterwards

afterwards detonated by flight friction. Next day, that is to fay twenty-four hours after the preparation, this young perfon was defirous of taking an equal quantity from the capfule to repeat the experiment, but he had fcarcely touched the preparation with the corner of a card when a violent detonation and explofion took place in the capfule which was fhattered into a thoufand pieces. His face was covered with the vaporifed preparation, which was almoft black, and adhered ftrongly to the fkin: his eyes experienced a ftrong fhock, which produced extreme pain; the opaque cornea became red and inflamed. Happily his fear was the greateft evil: by wafhing and bathing his eyes and face frequently with cold water, the effects of the detonation were foon diffipated.

Fortunately, none of the fragments of the glafs had touched his eyes or his face; they were thrown nearly in a horizontal direction, to a confiderable diftance: Some were thrown upwards of twelve feet.

As the effects of this preparation may have more calamitous confequences. I think it ufeful to be guarded againft the dangers which it may occafion.

XIV.

Pyrotechnic Obfervations, with their Application to evaporating Furnaces. By Cit.* CURAUDAU, *Correfponding Member of the Apothecaries Society of Paris, and Refident Affociate at the Atheneum of Arts.*

SINCE I publifhed my obfervations on the caufes of the imperfections of evaporating furnaces, I have had occafion to make others on the fame fubject, the application of which may add to the advantages in their conftruction which I have made known.

Difadvantages in the conftruction of evaporating furnaces, &c.

In my firft memoir, I proved that the bottom of the copper in the evaporating furnaces, not only obftructs the elevation of the temperature, but alfo diminifhes the activity of the fire, and is rather favourable to the gazification of the combuftible body, than to its oxigenation: I cited the lamp of Argand with its

* From Annales de Chimie, No. 149, Floreal, An. XII.

glafs

glaſs chimney, as an example of the neceſſity of raiſing the temperature round the combuſtible, whenever a compleat and powerful combuſtion is required. At preſent, I ſhall take the enameller's lamp as an example in ſupport of my new obſervations: I may ſay that I am indebted to the examination of its effects, for thoſe which it has led me to make. In fact, if the jet of the flame of an enameller's lamp be examined, it will be found that the intenſity of its action depends on the current of air which is directed on the flame of the wick; it will alſo be ſeen, that it is only at the extremity of its jet that the greateſt energy of the calorific rays exiſts, and that its intenſity is ſuch that, by means of this lamp, effects may be produced, which are, comparatively, as powerful as thoſe obtained in our beſt furnaces.

Effects of the current of air on the flame of Argand's lamp;

This mode of action of the caloric proves, therefore, that its effects may be augmented, by augmenting the rapadity of its current, and by directing it ſkilfully upon the body to be heated. Theſe are the conditions which I have endeavoured to unite in my new conſtruction, and which, agreeably to the application I have made of them, are employed to ſupport an opinion which required an experiment on a large ſcale to eſcape being placed in the claſs of hypotheſes.

Having been lately conſulted upon the conſtruction of a brewer's furnace, I took the opportunity to ſhow the great advantages of the alterations which my obſervations appeared to me to render neceſſary, and to induce the proprietor to conſtruct his furnace according to the plan I ſent him.

applied to works on a large ſcale.

The following is the reſult of the experiments which were made to aſcertain the advantages poſſeſſed by the new furnace over that which it replaced.

Compariſon of two furnaces.

The old furnace required 2¼ hours to raiſe the temperature of 2500 litres of well-water to 50° of Reaumur, and conſumed in the operation, which was repeated daily, 625 kilogrammes of new dry wood.

The preſent furnace, on the contrary, conſumes only 450 kilogrammes of wood in the ſame operation, and is only one hour in raiſing the temperature of 2500 litres of well-water to 50°: Whence it reſults, that this new conſtruction evidently makes a ſaving of $\frac{7}{11}$ in time and nearly one third in the combuſtible.

Such

Such advantages appeared to me to be of fufficient importance to deferve to be known, and to make it defirable that advantage may be taken of a new procefs which will have great influence on the economy of the fuel neceffary in manufactures.

Defcription of the Furnace. Plate

Defcription of the new furnace.

A. Opening of the fire-place: it is 16 inches wide and 13 high.

B. This part of the furnace refembles a vault: It is $21\frac{1}{2}$ inches in height, $31\frac{1}{2}$ in width, and 5 feet in depth. To add to the effect produced by the heat, I give the middle of the fire place a depth of 2 inches more that at its fides, which makes it preceptibly concave.

C. An opening made in the middle of the vault, and which is intended to increafe the rapidity and the action of the calorific rays; it is 5 inches high, and is equal in thicknefs to the vault. In its lower part, this opening is $23\frac{1}{2}$ inches long, and $17\frac{1}{2}$ wide; and, in its upper part, is $19\frac{1}{2}$ long, and $13\frac{1}{2}$ wide, which gives each extremity of the opening the form of a fpheroid whofe longeft axis is in the direction of the length of the vault.

D. The diftance of the copper from the orifice for the heat is 5 inches in the middle, and is reduced to 4, at the angle E, which gives the advantage of concentrating the calorific rays, in proportion as they lofe their intenfity by their expanfion.

From the angle E to that at F, there is a diftance of 15 inches: the re-entering angle of F is $2\frac{1}{4}$ inches from the copper, and its faliant angle only one inch.

G and H are angles fimilar to that at F, but which are 11 inches diftant from each other: thefe angles may be multiplied according to the height of the copper. The advantages which they give confift in making the calorific ftream undergo feveral breaks, which increafes their power at the place of their deviation.

I is an opening communicating with the chimney: it is 27 inches wide and 5 inches high. At the angle H, half the circumference of the furnace fhould be clofed by a row of bricks, for the purpofe of forcing the heat to direct itfelf from the

the ſide oppoſite to the chimney; immediately above this courſe of bricks, thoſe which ſucceed muſt be removed four inches from the copper, and continue ſo to the height of four inches; afterwards each courſe of bricks muſt be brought nearer, ſo that this reſervoir of heat may be cloſed at the height of five inches, which muſt be done round all the circumference of the copper.

K is the opening of the chimney: it muſt be three decimetres ſquare through all its height, and at leaſt four metres high.

L is a blower of hammered iron; it is one metre above the copper, and ſerves to open or cloſe the chimney at pleaſure.

The proportions laid down in this plan are intended for a copper four feet three inches wide in its lower part, and forty inches deep.

XV.

An Account of a curious Phenomenon obſerved on the Glaciers of Chamouny; together with ſome occaſional Obſervations concerning the Propagation of Heat in Fluids. By BENJAMIN *Count of* RUMFORD, *V. P. R. S. Foreign Aſſociate of the National Inſtitute of France, &c. &c.* *

Cylindrical pit (in a maſs or ſea of ice) containing water.

IN an excurſion which I made the laſt ſummer, in the month of Auguſt, to the Glaciers of Chamouny, in company with Profeſſor Pictet of Geneva, I had an opportunity of obſerving, on what is called the Sea of Ice, *(Mer de Glace,)* a phenomenon very common, as I was told, in thoſe high and cold regions, but which was perfectly new to me, and engaged all my attention. At the ſurface of a ſolid maſs of ice, of vaſt thickneſs and extent, we diſcovered a pit, perfectly cylindrical, about ſeven inches in diameter, and more than four feet deep; quite full of water. On examining it on the inſide, with a pole, I found that its ſides were poliſhed; and that its bottom was hemiſpherical, and well defined.

This pit was not quite perpendicular to the plane of the horizon, but inclined a little towards the ſouth, as it deſcended;

* Phil. Tranſ. 1804, p. 23.

fcended; and, in confequence of this inclination, its mouth or opening, at the furface of the ice, was not circular, but elliptical.

They are frequently found.

From our guides I learnt, that thefe cylindrical holes are frequently found on the level parts of the ice; that they are formed during the fummer, increafing gradually in depth, as long as the hot weather continues; but that they are frozen up, and difappear, on the return of winter.

Inference againft the conducting power of water.

I would afk thofe who maintain that water is a conductor of heat, how thefe pits are formed? On a fuppofition that there is no direct communication of heat between neighbouring particles of that fluid, which happen to be at different degrees of temperature, the phenomenon may eafily be explained; but it appears to me to be inexplicable on any other fuppofition.

The quiefcent mafs of water, by which the pit remains conftantly filled, muft neceffarily be at the temperature of freezing; for it is furrounded on every fide by ice: but the pit goes on to increafe in depth, during the whole fummer. From whence comes the heat that melts the ice continually at the bottom of the pit? and how does it happen, that this heat acts on the *bottom* of the pit only, and not on its fides?

Solution of the effect by the greater denfity of water above zero.

Thefe curious phenomena may, I think, be explained in the following manner: The warm winds which, in fummer, blow over the furface of this column of ice-cold water, muft undoubtedly communicate fome fmall degree of heat to thofe particles of the fluid with which this warm air comes into immediate contact; and the particles of the water at the furface fo heated, being rendered fpecifically heavier than they were before, by this fmall increafe of temperature, fink flowly to the bottom of the pit; where they come into contact with the ice, and communicate to it the heat by which the depth of the pit is continually increafed.

This operation is exactly fimilar to that which took place in one of my experiments, (See my Effay on the Propagation of heat in Fluids, *Experiment* 17,) the refults of which, no perfon, to my knowledge, has yet explained.

Conftant temperature of water at the bottom of deep lakes, offered in fupport of the non-

There is another very curious natural phenomenon, which I could wifh to fee explained in a fatisfactory manner, by thofe who ftill refufe their affent to the opinions I have been led to adopt, refpecting the manner in which heat is propagated

in

in fluids. The water at the bottoms of all deep lakes is conſtantly at the ſame temperature, (that of 41° Fahrenheit,) ſummer and winter, without any ſenſible variation. This fact alone appears to me to be quite ſufficient to prove, that if there be any immediate communication of heat between neighbouring particles or molecules of water, *de proche en proche*, or from one of them to the other, that communication muſt be ſo extremely ſlow, that we may with ſafety conſider it as having no exiſtence; and it is with this limitation that I beg to be underſtood, when I ſpeak of fluids as being non-conductors of heat.

conducting property of fluids.

In treating of the propagation of heat in fluids, I have hitherto confined myſelf to the inveſtigation of the ſimple matter of fact, without venturing to offer any conjectures relative to the cauſes of the phenomena obſerved. But the reſults of recent experiments on the calorific and frigorific radiations of hot and of cold bodies, (an account of which I ſhall have the honour of laying before the Royal Society in a ſhort time,) have given me ſome new light reſpecting the nature of heat, and the mode of its communication; and I have hopes of being able to ſhow *why* all changes of temperature, in *tranſparent* liquids, muſt neceſſarily take place at their ſurfaces.

Notice of experiments againſt this doctrine.

I have ſeen with real pleaſure, that ſeveral ingenious gentlemen, in London, and in Edinburgh, have undertaken the inveſtigation of the phenomena of the propagation of heat in fluids; and that they have made a number of new and ingenious experiments, with a view to the farther elucidation of that moſt intereſting ſubject. If I have hitherto abſtained from taking public notice of their obſervations on the opinion I have advanced on that ſubject, in my different publications, it was not from any want of reſpect for thoſe gentlemen that I remained ſilent, but becauſe I ſtill found it to be quite impoſſible to explain the reſults of my own experiments, on any other principles than thoſe which, on the moſt mature and diſpaſſionate deliberation, I had been induced to adopt; and becauſe my own experiments appeared to me to be quite as concluſive (to ſay no more of them) as thoſe which were oppoſed to them; and, laſtly, becauſe I conſidered the principal point in diſpute, relative to the paſſage of heat in fluids, as being ſo clearly eſtabliſhed by the circumſtances attending ſeveral great operations of nature, that this evidence did not appear to me to be in

 danger

danger of being invalidated by conclufions drawn from partial and imperfect experiments, and particularly from fuch as are allowed on all hands to be extremely delicate.

Heat of the fides of the veffel, and defcending current in a veffel of ice.

In all our attempts to caufe heat to defcend in liquids, the heat unavoidably communicated to the fides of the containing veffel, muft occafion great uncertainty with refpect to the refults of the experiment; and, when that veffel is conftructed of ice, the flowing down of the water refulting from the thawing of that ice, will caufe motions in the liquid, and confequently inaccuracies of ftill greater moment, as I have found from my own experience; and, when thermometers immerfed in a liquid, at a fmall diftance below its furface, acquire heat, in confequence of a hot body being applied to the furface of the liquid, that event is no decifive proof that the heat acquired by the thermometer is communicated by the fluid, from above, downwards, from molecule to molecule, *de proche en proche;* fo far from being fo, it is not even a proof that it is from the fluid that the thermometer receives the heat which it acquires; for it is poffible, for aught we know to the contrary, that it may be occafioned by the radiation of the hot body placed at the furface of the fluid.

Reference to the experiments of boiling water ftanding over ice.

In the experiments of which I have given an account, in my Effay on the Propagation of Heat in Fluids, great maffes, many pounds in weight, of boiling hot water, were made to repofe for a long time (three hours) on a cake of ice, without melting but a very fmall portion of it; and, on repeating the experiment with an equal quantity of very cold water, (namely, at the temperature of 41° Fahrenheit,) nearly twice as much ice was melted in the fame time. In thefe experiments, the caufes of uncertainty above mentioned did not exift: and the refults of them were certainly moft ftriking.

The conclufions which naturally flow from thofe refults, have always appeared to me to be fo perfectly evident and indifputable, as to ftand in no need, either of elucidation, or of farther proof.

If water be a conductor of heat, how did it happen that the heat in the boiling water did not, in three hours, find its way downwards, to the cake of ice, on which it repofed, and from which it was feparated only by a ftratum of cold water, half an inch in thicknefs?

I wifh

I wifh that gentlemen who refufe their affent to the opinions I have advanced refpecting the caufes of this curious phenomenon, would give a better explanation of it than that which I have ventured to offer. I could likewife wifh that they would inform us how it happens, that the water at the bottoms of all deep lakes remains conftantly at the fame temperature: and, above all, how the cylindrical pits, above defcribed, are formed in the immenfe maffes of folid and compact ice which compofe the Glaciers of Chamouny! Recapitulatory arguments.

A remark, which furprifed me not a little, has been made by a gentleman of Edinburgh, (Dr. Thompfon,) on the experiments I contrived, to render vifible the currents into which liquids are thrown on a fudden application of heat, or of cold. He conceives, that the motions obferved in my experiments, among the fmall pieces of amber which were fufpended in a weak folution of potafh in water, were no proof of currents exifting in that liquid; as they might, in his opinion, have been occafioned by a change of fpecific gravity in the amber, or by air attached to it. I am forry that fo mean an opinion of my accuracy as an obferver fhould have been entertained, as to imagine that I could have been fo eafily deceived. For nothing furely is eafier, than to diftinguifh the motion of a folid fufpended in a liquid of the fame fpecific gravity, which is carried along by a current in the liquid, from that of a body which defcends, or afcends, in the liquid, in confequence of its relative weight, or levity. In the one cafe, the motion is uniform; in the other, it is accelerated. In a current, the body may be carried forward in all directions, and even in curved lines; but, when it falls in a quiefcent fluid, by the action of gravity, or rifes, in confequence of its being fpecifically lighter than the fluid, it muft neceffarily move in a vertical direction. Notice of one remark of Dr. Thompfon.

The fact is, that I very often obferved, in the courfe of my numerous experiments, the motions of fmall particles of matter, of different kinds, in water, which Dr. Thompfon defcribes; but, fo far from inferring *from them* the exiftence of currents in that fluid, their caufe was fo perfectly evident, that I did not even think it neceffary to make any mention of them.

I cannot conclude this Paper, without requefting that the Royal Society would excufe the liberty I have taken in troubling

 them

them with these remarks. Very desirous of avoiding every species of altercation, I have hitherto cautiously abstained from engaging in literary disputes; and I shall most certainly endeavour to avoid them in future.

I am responsible to the public for the accuracy of the accounts which I have published of my experiments; but it cannot reasonably be expected, that I should answer all the objections that may be made to the conclusions which I have drawn from them. It will however, at all times, afford me real satisfaction to see my opinions examined, and my mistakes corrected; for my first and most earnest wish is, to contribute to the advancement of useful knowledge.

XVI.

Account of two Sketches; viz. *one for a perpetual Motion, and the other of a jointed Parallel Rule, which has no side Deviation. In a Letter from* R. B.

To Mr. NICHOLSON.

SIR,

Perpetual motions described in the Philosophical Journal.

I was much gratified several years ago by some essays with which you obliged your readers upon the perpetual motion. In the first volume, p. 375, of your quarto series, I find an account of several schemes (necessarily abortive) for producing perpetual motions by the action of gravity, and in your third volume, I find an account of various methods of keeping up the motion of a machine by means of the changes which take place in the barometer and thermometer, I have ventured to send you the sketch of a project for a perpetual motion of the latter kind, which has long remained among my memorandums. You will see that it is not of the class of perpetual motions properly speaking, but merely the application of some existing intermittent motions in nature to the purpose of maintaining the rotation of machinery. If you should hesitate about inserting this, I think the other sketch which accompanies it of an useful instrument, cannot fail to meet your approbation.

Description of a wheel kept in motion by the marine barometer.

Fig. 1, *Plate XII.* A represents the marine barometer of Halley, but varied by the addition of a vessel B at the open end, in which the water or other fluid, exposes a surface nearly equal

equal to that in the closed vessel A. These two vessels are connected by a long horizontal tube G. It is evident that any change, either in the pressure of the external air or the elasticity of the internal air, will cause the fluid to run along the tube, and add to the weight of A or of B according to circumstances. The heavier vessel will preponderate; but it will be prevented from descending too far by a stop or bearing to which it will arrive. Any change in the inclination of G will move the attached lever C D; by means of which, one of the two horizontal racks will be made to push round that ratchett wheel into which its teeth fall, at the same time that the other rack will be drawn backwards upon its wheel. The opposite action will drive forward the other wheel; and as both these wheels are fixed on the same axis, the system will be driven the same way by every change of density or weight in the air that takes place.

Account of a new parallel rule.

The other instrument, *Fig.* 3, is a parallel rule which is framed without any sliding work, and opens to a much greater extent than usual, without any side deviation. I do not know the inventor; but it was communicated to me by a private hand. The jointed parts are represented in *Fig.* 2, and the dimensions are expressed by the small numerals. From the joint D to the joint E upon the bar, C E, the distance, is = 1; from the joint E to the joint A, = 2; from the joint A to the joint B, = 3; and from the joint B to the joint D, = 4; which is also equal to the part D C of the Bar E C. Now it is found, and I must leave the mathematical proof to your readers, that the point C moves by the opening from B (upon which it lies when shut) in a strait line, very nearly at right angles with B A. From this property it is easily seen, that two parallel rules, having the system of four bars fixed to each, and connected as in the figure, will be moveable without side deviation, and will preserve their parallelism in all situations.

I am Sir,

Your obliged humble servant,

R. B.

Familiar

XVII.

Familiar Account of the Method of eſtimating the Value of a Steam-Engine in Horſe-Powers as they are called. By a Correſpondent.

To Mr. NICHOLSON.

SIR,

Introductory letter.

AS your excellent Journal is the repoſitory for uſeful information, whether ſcientific or practical, I have thought I ſhould oblige many manufacturers and others of your readers, by ſending you a very clear report about horſe-powers, which a friend of mine has communicated to me, and was received by him from an eminent character in anſwer to an enquiry profeſſionally made.

It clearly appears from this paper, that the calculation by horſe-powers muſt be fallacious, unleſs engineers could agree as to the quantity of work they would arbitrarily, in the firſt inſtance, aſcribe to one horſe; and then the expreſſion would be nugatory. And not only ſo, but it would not then be true that the value of a ſteam-engine in work, however clearly expreſſed in quantity *per* day, would be fairly ſhewn, unleſs the wages or food of the working being were taken into the account. Coals may be ſtiled the food of a ſteam-engine, and nothing is more evident than that, if two engines raiſe equal quantities of water *per* hour, but conſume different quantities of coal, they will not be equally beneficial to the proprietors. I would therefore propoſe, that the eſtimate ſhould be made by attending to theſe two particulars only, and ſaying nothing about horſes, at leaſt in ſpecific arguments intended to have legal effects: And, as a ſupplement to the facts and obſervations contained in the report, I will add, that one of the beſt engines of Boulton and Watt, has been known to raiſe between 28 and 30 millions of pounds of water to the height of one foot with one buſhel of good coals, which appears to be an outſide meaſure; and that, though there are ſubſequent improvements both in the conſtruction of furnaces and the working gear, yet there are ſome among late engines which fall ſhort of 20 millions.

I am, Sir,

Your conſtant reader,

E. T.

Report

*Report concerning the Power of a Steam-Engine erected by Contract at * * * *.*

IT is required to determine whether the fteam-engine erected at * * * * by Mr. ——, be equal to the power of fixteen horfes. The fame has a cylinder of $21\frac{1}{8}$ inches diameter, and gives 23 double ftrokes *per* minute, of four feet each.

Steam engine at * * * *.

In anfwer to this queftion it muft be previoufly remarked, that fteam-engines having originally been recommended and fubftituted inftead of horfes, the method of computing by the number of thofe animals intended to be fupplied by means of this invention, has been generally applied, though it is much lefs certain and accurate than other methods well known to mechanical men. The uncertainty of calculating by horfe-powers arifes from various caufes; fuch as the great differences of ability between the ftrong and heavy horfes ufed in London, and thofe of not half the ftrength ufed in various parts of the country; the greater or lefs degree of fpeed during work; the quantity of re-action againft which they are urged to pull; the fhorter or longer time of work; their food, ftabling, &c. &c. And this uncertainty, as may eafily be conceived, is fo great, that the words *horfe-power* cannot practically be applied, otherwife than to denote a certain quantity of mechanic effect agreed upon and underftood between engineers, and muft not be underftood to denote any elementary meafure, capable of being worked out or inferred within any reafonable or ufeful limits, from the real power of the horfe himfelf.

Reafon why fteam engines have been compared with horfes.

It is an uncertain method.

It therefore follows of neceffity, that the engine muft be examined by firft ftating its mechanic effect; that is to fay, how many pounds weight it is capable of raifing through a given fpace in a given time, that is to fay, through the height of one foot during one minute, and then dividing this fum by the like effect producible by one horfe, according to the ftatements and practice of engineers of the firft reputation. I confine myfelf to Meff. Boulton and Watt, Dr. Defaguliers and Mr. Smeaton.

Words horfe-power muft denote an arbitrary quantity of effect.

The practice of Meff. Boulton and Watt is, to confider a horfe as capable of raifing a certain weight, which is ftated to be of 32000 pounds avoirdupois, one foot high in one minute. Defaguliers' refults brought to the fame form, give 27500 pounds;

Quantities of work per horfe, by Boulton and Watt, Defaguliers and Smeaton.

pounds; and Smeaton's 22916 pounds, under the ſame circumſtances. The loweſt of theſe performances are more than equal to the average power of a horſe employed in huſbandry for eight hours *per* day.

A ſteam engine computed.

If the diameter of the cylinder be multiplied by itſelf ($21\frac{1}{8} \times 21\frac{1}{8}$), the product will give $446\frac{1}{4}$ round inches for the whole ſurface: and Smeaton reckons the effective or working preſſure *per* round inch on the atmoſpheric engine, at ſeven pounds avoirdupois. It is uſual to reckon the working preſſure on a cloſe engine (like that in queſtion), at 10 pounds the round inch; but I ſhall firſt take the ſeven pounds as being againſt the builder: So that, by multiplying the round inches $446\frac{1}{4}$ by 7, we have $3123\frac{3}{4}$ pounds for the weight raiſed. But the ſtrokes are 23 of four feet double *per* minute, that is to ſay, 184 feet. Multiply the weight $3123\frac{3}{4}$ by the height 184, and the product 574760 will be the mechanic effect of the engine, or the number of pounds it will raiſe one foot high in one minute. Laſtly, Divide this by Boulton and Watt's horſe-power ($\frac{574760}{32000}$), and the quotient $17\frac{15}{16}$, or very nearly 18, will expreſs the power of the engine in horſes.

If we follow Deſaguliers, the engine will prove equal to 21 horſes.

And, according to Smeaton, its power will be equal to 25 horſes.

If we had taken the preſſure at 10 pounds *per* round inch, the powers would have proved much greater, as below.

Steam engine can work treble days.

In the above calculations the horſes are ſuppoſed to be fairly worked, and the engine is ſuppoſed to be ſtopped as ſoon as the horſes leave off. But an engine can work the whole 24 hours; and Smeaton, conſidering that three ſetts of horſes muſt be kept to work conſtantly for the ſame time, reckoned a ſteam engine to be equivalent to three times as many horſes as could equal its rate of working. The following table will ſhew the powers of this engine, according to all theſe ſeveral methods:

7 lb.

	Power in Horfes according to Boulton and Watt.	Ditto according Defaguliers.	Ditto according to Smeaton.	24 hours work according to Smeaton in horfes.
7lb. preffure per round inch.	$17\frac{96}{100}$ or 18 very nearly	21	25	75
10lb. preffure per round inch.	25	30	$35\frac{3}{4}$	107

XVIII.

Experiments proving the Neceffity of atmofpherical Oxigen in the Procefs of Vegetation. In a Letter from Mr. JOHN GOUGH.

To Mr. NICHOLSON.

SIR,

DR. Prieftley and M. Ingenhoufz have made many experiments with plants, confined in azote; from which philofophers have inferred, that vegetables differ from animals in not requiring the prefence and fupport of atmofpherical oxigen. One circumftance however occurs in all thefe experiments, if my memory be correct, which renders the conclufion doubtful: the plants, ufed in them were in contact with the water that confined the azote in the inverted jars, through the medium of which they had evidently the fame indirect communication with the atmofphere that fifhes are known to require.

Reflections on former experiments of the kind.

With a view to throw fome additional light upon this difputable point in the vegetable economy, I determined to furround certain fucculent plants entirely with azote; which poffefs the fingular property of vegetating when fufpended by the roots in dry air. My method of performing this confifts in fixing the fubject of the experiment by means of a thread, to a fmall perforated board, ftanding on a pillar, which was furnifhed with a foot of lead. The plant being thus fecured, was introduced through water into a glafs jar of azote; where it was wholly furrounded by the gas.

Experiments on fucculent vegetables.

Exp. 1. The firft experiment, I made in this manner, was tried in June 1800. A plant of fedum acre was furrounded by azote when it was ready to expand its flowers, and another was placed befide it, under a fimilar jar; which contained

Experiment with fedum acre going to flower.

common air. The latter ſpecimen flowered very well; but the former died away without opening one of its bloſſoms.

With the ſame in flower.

Exp. 2. The event was the ſame when I made uſe of a branch of this ſedum already in flower; for the plant withered and did not ſhed its petals; which is contrary to the uſual habits of this vegetable, when it is left to the impulſes of nature unthwarted by art.

With barren plants of the ſame and ſempervivum tectorum.

Exp. 3. Several plants of ſedum acre were alſo confined in jars filled with azote laſt ſpring, before the flower buds were formed upon them: not one of theſe attempted to vegetate; on the contrary they all withered in the courſe of four or five weeks. This was not the caſe with other ſpecimens, which were placed beſide them upon ſimilar ſtands, under jars containing common air and ſtanding in water; for they remained green and vigorous, after the former were dead; they had moreover produced a number of purple filaments ſpringing from the ſtalk; which is conſtantly done by the ſedum acre, when ſuſpended by the roots. It is proper to remark, that the air of the latter ſet was frequently changed, with a view to keep it near the ſtandard of the atmoſphere. The *ſempervivum tectorum* did not offer to vegetate when treated in the ſame manner by being confined in azote; but it appeared to be more tenacious of life under this kind of treatment, than the ſedum acre.

Inference from theſe experiments.

The preceding experiments induced me to conclude, at the time of making them, that the welfare of ſucculent plants requires them to be expoſed to the atmoſphere, and that they cannot grow when ſurrounded by azote, nor even preſerve the vegetative principle in it, for an indefinite length of time.

Seeds and bulbs in the ſame predicament.

I have ſhewn in the fourth volume of the Mancheſter Memoirs, that ſeeds and bulbs are not leſs indebted for their proſperity to the agency of oxigen, than ſucculent plants appear to be from the preſent letter. Thus it is manifeſt that the farther we carry our reſearches into the nature of organized bodies, the greater is the number of them; which are found to be incapable of performing their natural functions, when deprived of reſpirable air; but as the truth of this maxim remains to be extended experimentally to vegetables of a leſs humid conſtitution, than the ſedums, the following fact may be related with propriety.

Experiment made with ceraſtium vulgatum,

Exp. 4. A diminutive plant of *ceraſtium vulgatum* was placed on the 18th of February, 1804, upon a ſtand covered with a

ſlice of wet ſponge; it was then put under an inverted jar, containing common air, and ſtanding in water. This plant encreaſed conſiderably in bulk in the courſe of 20 days; beſides which it had taken root in the ſponge; it was therefore transferred into a jar filled with azote; but this change of circumſtances put a ſtop to vegetation; for the herb made no further advances, but withered away.

Exp. 5. In order to diverſify theſe experiments as much as poſſible, I took a number of phials, having narrow necks, and filled them with water, which was either procured from ſnow, or deprived of air by boiling. Proper plants being then placed in the bottles, they were introduced quickly through water into jars filled with azote; under which circumſtances, all my ſpecimens died away ſooner or later without making one effort to vegetate. The herbs uſed on theſe occaſions were the common garden ſpear-mint and the draco-cephalum Moldavicum; both of which vegetate freely in bottles of water expoſed to the atmoſphere. I alſo treated in the ſame manner, a branch of Lyſimachia vulgaris, which was ready to flower; it declined gradually for a month, but never opened one bloſſom; though it is an aquatic plant and flowers very well in jars of water. It may here be remarked once for all, that vegetables die very ſlowly in azote, provided they are ſupplied with water deprived of air; but if my experiments may be relied on, all their natural functions ceaſe in theſe circumſtances, as ſoon as they are removed from the atmoſphere.

with plants in water deprived of air.

Exp. 6. The loſs of energy experienced by plants confined in mephitic air, does not appear to ariſe from an injurious quality of the gas, but from the want of the ſalutary ſtimulus of oxigen; for a ſlip of ſpear-mint, which had remained twelve days in a glaſs of azote, recovered upon being reſtored to the air, with the loſs of its loweſt leaves only; and an off-ſet of ſempervivum vegetated freely after being removed from a jar of the ſame gas, in which I had kept it, from the 2nd of April to the 2nd of May, ſtanding in a bottle of ſnow water. Theſe facts ſeem to argue, that vegetables, treated in the manner deſcribed as above, ſuffer a ſpecies of torpidity, from which they may be recalled by the preſence of atmoſpherical air, provided the re-admiſſion of this neceſſary agent be not delayed too long; in which caſe, the plant periſhes, in conſequence of the

Plants die in azote for want of their proper ſtimulus.

the continued inactivity of its organs, and the fermentation of its juices; which is occasioned by the suspension of all its functions.

Succulent plants absorb air from the atmosphere, but dry ones from water.

This fluid so necessary to the process of vegetation, is evidently absorbed from the atmosphere by succulent plants, bulbs, and a variety of seeds, when properly moistened; for they will all vegetate freely in open situations; but the following experiment seems to prove that trees, shrubs and the less juicy herbs derive air from water, as fishes do.

Exp. 7. I introduced the lower extremities of different plants, or more properly of the branches of different plants, into phials filled with water, and after luting the necks with wax, I sunk the bottles in pots of water, so as to leave the foliage of the plants exposed to the sun and surrounded by the atmosphere. The evaporation caused by the action of the light drew the water out of the bottles, and the air descending at the same time through the substance of the plants prevented a vacuum being formed in these close vessels. This fact had nearly persuaded me, that vegetables possess a faculty analogous to respiration; but when I repeated the experiment with phials perforated in the bottom, they remained full of water. This circumstance shews the respiration in question to be merely accidental and to be occasioned by the necks of the bottles being luted; on the other hand it is highly probable, that air enters the vessels of vegetables in conjunction with water. Here it is decompound, in consequence of the stimulus given to the vegetative principle by its presence; the azote is afterwards assimulated into the substance of the plant, while the oxigen is rejected either wholly or in part. This gas, after its expulsion flows into the mephitic air, contained in the jars, the volume of which is thereby augmented, and its quality improved; but I will not venture to say, that the two gases form a new chemical compound on these occasions.

Exception to the general rule that plants require air.

Exp. 8. I am acquainted with a single exception to the general rule that air is requisite in the process of vegetation. For when plants have withered in azote they become mouldy; that is, in the language of botany, they are covered with the mucor mucedo. The worms which inhabit the decayed livers of sheep, &c. furnish us with a parallel instance in the animal kingdom; and the investigation of these singular anomalies forms a difficult problem in the natural history of organized bodies.

The

The ſentiments contained in this letter, are oppoſite to the common notions relative to the nature of vegetation, and ſo manifeſt a departure from eſtabliſhed opinions may be thought to require an apology. The juſtification of my conduct however appears in the body of the eſſay. The experiments are related with an attention to circumſtances; which will enable almoſt any perſon to repeat them; and ſhould ſome philoſopher undertake to do it, his labours will either eſtabliſh my opinions, or confirm the contrary doctrine, by the refutation of them. There is one omiſſion in the detail of theſe experiments; I have neglected to remark, that the term azote ſignifies nothing more than common air deprived of oxigen by the ſulphuret of potaſh or the martial paſte of ſulphur, but more frequently by the former. The determination of the queſtion is of ſome moment, not only to natural hiſtory, but perhaps to agriculture alſo; and I could wiſh it to be reſumed by a philoſopher, whoſe advantages are ſuperior to my contracted powers of obſervation and imperfect apparatus. Should any man of ſcience undertake the taſk, I would recommend the ſedum acre to his notice; becauſe the filaments which ſpring from the ſtalk of this plant in common air afford a criterion whereby the progreſs of ſimilar ſpecimens confined in azote may be obſerved and aſcertained.

The eſſay concluded.

JOHN GOUGH.

Middleſhaw,
October 24, 1804.

SCIENTIFIC NEWS, ACCOUNT OF BOOKS, &c.

Frothing of Oil by Electricity.

THE anonymous author of ſome electrical experiments communicated to Van Mons the following fact, from whoſe Journal I tranſlate it. He was making an experiment to aſcertain the conducting quality of oil, for which purpoſe he had filled one third part of the length of a tube with the oil of Colſa. The tube was three feet long and half an inch in diameter, having its cloſed end drawn out. The open extremity was ſecured by a cork, through which a needle was thruſt; and over the cork was a coating of wax to prevent the oil from tranſuding. The tube

tube being then placed on two infulating fupports, and the needle turned towards the part moft remote from the machine, the tube was flightly inclined, in order that the oil might run down to that part of its capacity which was neareft the prime conductor. He then put the machine in action, and with great furprife obferved the oil gradually to enter into a ftate of ebullition; beginning at the furface and fpeedily acquiring a ftate of effervefcence perfectly refembling that of fparkling Champaigne. Upon opening the tube, a gas efcaped with violence, which the author did not collect, but which well deferves to be examined. This active electrician intends to fubject the different vegetable and mineral acids to the action of electricity by the fame procefs.

Native Magnefia.

GIOBERT has found that a white earth confidered as pure alumine and employed at Turin in the fabrication of porcelain, contains 0,80 of magnefia. VAN MONS.

Gray's Experienced Mill-wright.

The Experienced Mill-wright; or a Treatife on the Conftruction of fome of the moft ufeful Machines, with the lateft Improvements. To which is prefixed a fhort Account of the general Principles of Mechanics and of the Mechanical Powers. Illuftrated with forty Engravings. By Andrew Gray, *Millwright, very large quarto, 73 Pages of Letter Prefs, of the fame Size as the Engravings.*

THE author is a practical mechanic, who has been upwards of forty years employed in erecting different kinds of machinery; and the greateft part of the Defcriptions in this Work are of Machines which he has either planned or fuperintended their conftruction. Thefe Drawings being accurately made to fcale, are not, as he remarks, to be confidered as mechanical fpeculations, always doubtful till fubjected to actual experiment, but as cafes of practical knowledge, the effects of which have been fully tried and long approved; moft of them being ftill employed for the purpofes of their original fabrication.

Previous to his Account of the Machines, this author gives a popular Treatife on Mechanics, in fix different chapters, beginning with General Principles and Definitions, which are followed

lowed by Explanations of Simple Engines, called the Mechanical Powers. Thefe are fucceeded by an Account of the Effects of Friction, and the beft Method of applying thofe Mechanical Powers. Practical Directions for defigning and conftructing the Parts of Machines are in the next place given; and laftly, the author treats on the Strength of Machines, the relative Velocity of their Parts and the leading Doctrines which are neceffary to be attended to in the erecting of Water-Mills.

The Machines of which Meafured Plans, Elevations and Sections are given, are; A confiderable number of Thrafhing Machines, to be drawn either by Water or by Horfes, with the Elevation of a Wind-mill to turn Machinery of this Defcription; one Snuff Mill; two Sheeling Mills; feveral Corn, Barley, Malt, and Flour Mills; one Oil Mill; one Flax Mill; one Paper Mill; one Saw Mill; one Fulling Mill; Bleaching Machines, Beetling Machines, and various Pumps.

Thefe Drawings and Defcriptions will no doubt prove of high value to the man of bufinefs; though it is perhaps to be regretted that the fame attention to junior ftudents which is difplayed in the introductory treatife, is not as fully fhewn by explaining the relative operations and effects of the parts of thefe machines in the defcriptions which accompany the drawings.

Practical Obfervations concerning Sea Bathing; to which are added, Remarks on the Ufe of the Warm Bath. By A. P. Buchan, *M. D. of the Royal College of Phyficians. London. Crown* 8*vo.* 207 *Pages.*

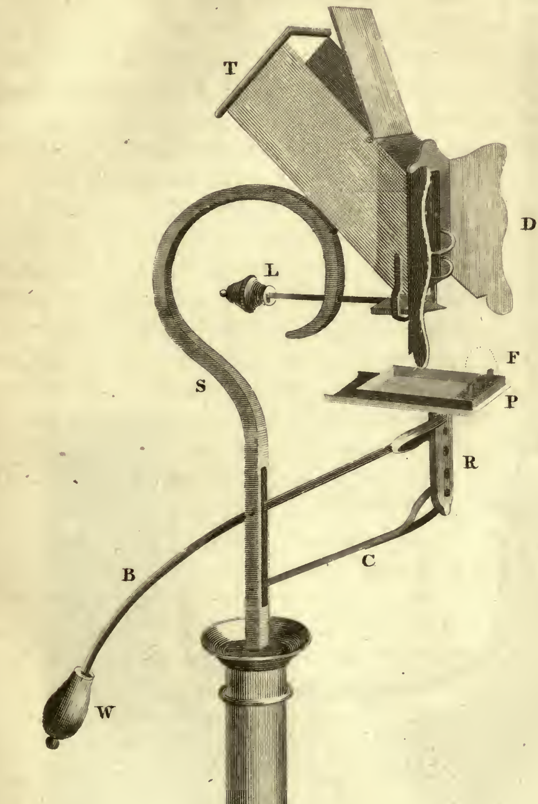
IN the early ftages of human fociety, when the occupations of man led him to ftrong exercife in the open air, it is probable that his health may have been chiefly impaired by the fortuitous occurrences of his mode of life, and the direct wear of his organs engaged in the grofs and active labours of his life. It is probable that he may have felt little of thofe difeafes which the claffification and habits of a refined and luxurious fociety have introduced. Neither mental fatigue, nor the anxious and inceffant labour of crowded cities could have operated upon his powers, nor was he fubjected to the miferies of indolence, repletion, and the liftlefs want of motive which benumb the faculties of the rich. Thefe are the prices which by Buchan on fea bathing.

Buchan on sea bathing.

by our numerous mistakes we are compelled to pay for the superior enjoyments which an enlightened state of society can afford to those only who possess the virtue, the conduct, and the good fortune to know and to acquire them. Among the most oppressive of these consequences, no doubt, is the loss of health; and while we make sacrifices to what we imagine to be our welfare, it becomes us occasionally to interrupt our usual train of habits, to unbend the mind, give repose to the system, and recover, by an occasional change of our occupations, that vigour which sameness of pursuits and confinement seldom fail to impair.

One of the means at present much resorted to is, to repair to the sea-coast; partly with a view to enjoy a scene which, by its impressive magnitude and the numerous changes it undergoes, cannot but be highly interesting; but principally with the intention of bathing in the sea. Bathing is certainly of the greatest benefit in many cases, and, like every other practice capable of strongly affecting the human frame, it must also, in other cases be capable of operating to the injury of the health. We have no direct general treatise on this subject; and to give the requisite information to individuals by which they may safely and judiciously regulate themselves requires somewhat more than mere medical skill. The present work abounds with interesting facts and clear deductions, and is written with that candid spirit of philosophical remark which shews an intimate acquaintance with the moral as well as the physical conditions of our existence, and the accidents to which we are exposed. I am much mistaken if any one who begins to read this work will be disposed to quit it without perusing the whole; and there are few who can have so extensively considered the subject, or who are so little concerned in its discussion as not to derive advantage from the many points of information it contains.

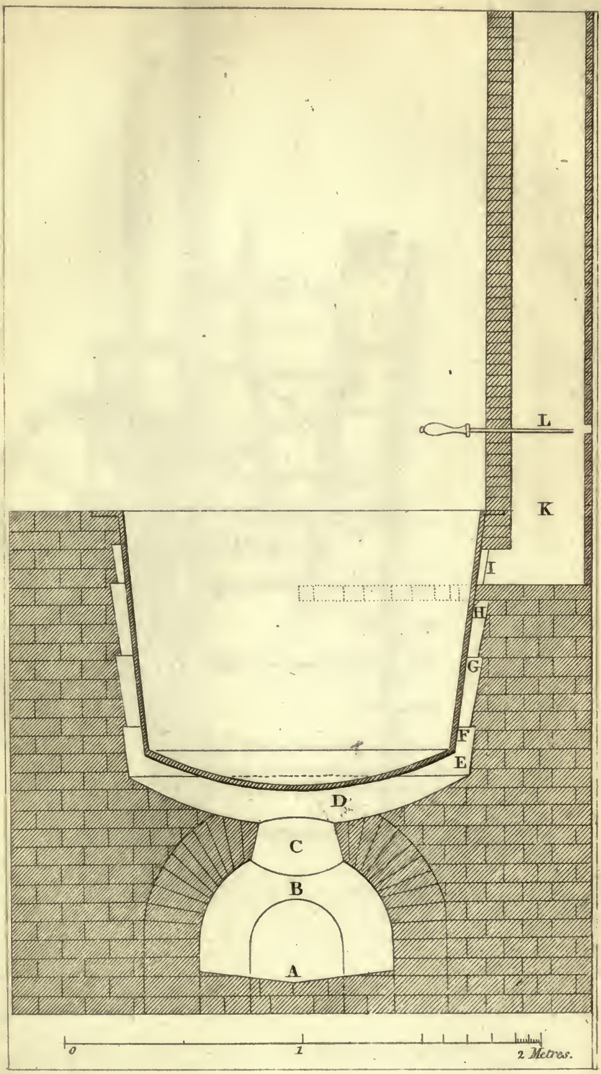
Tallow Lamp by Mr. J. Wm. Boswell.



Drawn by Blunt.

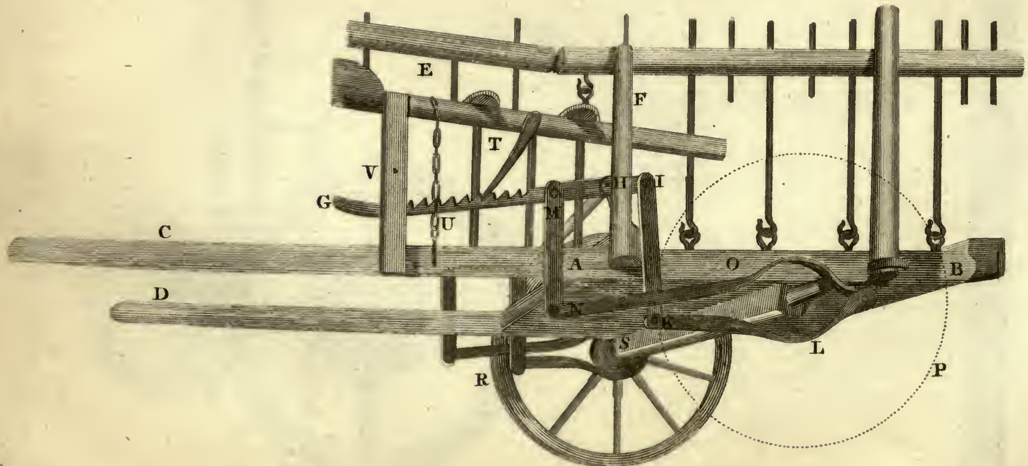
Engraved by Mutlow.

Evaporating Furnace by Curaudau.



Mutlow Sc. Russell Ct.

Mr. Wm. Bowler's double Brace for Carriages.



Mutlow Sc. Russell Ct.

Perpetual Motion.

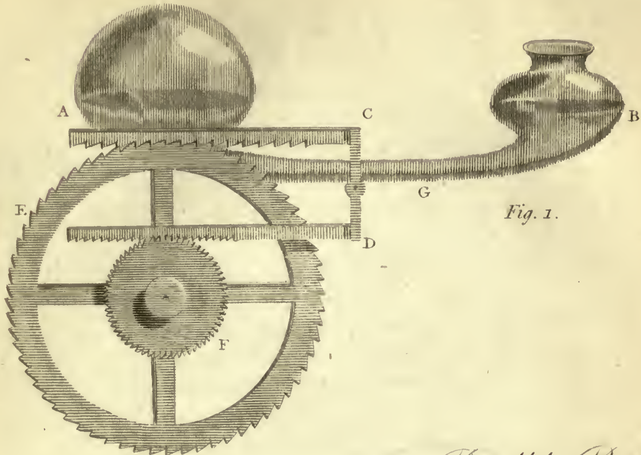


Fig. 1.

Parallel Ruler.

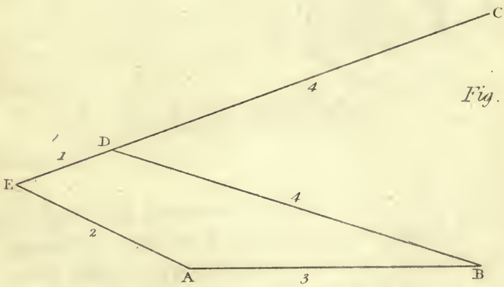
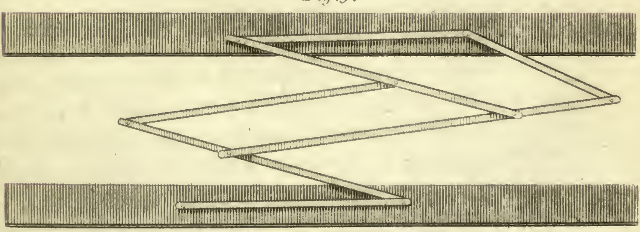


Fig. 2.

Fig. 3.



A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

DECEMBER, 1804.

ARTICLE I.

Description of a Tubular Pendulum; having all the Properties of the Gridiron; but being more compact as well as more steady in its Motions. In a Letter from Mr. EDWARD TROUGHTON, *the Inventor.*

To Mr. NICHOLSON.

DEAR SIR,

EVER since the commencement of your Journal, I have often thought of registering therein, the improvements which I frequently make in astronomical and other instruments. No man perhaps had ever a greater antipathy to writing than myself, and drawn as I am into the vertex of business, and working with my own hands from nine to twelve hours in a day, leaves me but little time for literary pursuits; yet in one who values himself on account of the originality of his ideas, it must seem slothful not to commit them to the press, which until that is done, are like flowers or fruit growing fenceless by the way side, which every one that passes by may gather.

Advantages of publishing improvements in philosophical apparatus.

I always considered the pendulum of a clock more suitable to our stile of workmanship and habits of thinking, than to those of any other description of artists; and agreeable to this idea, I have made pendulums near twenty years, and I own the

Pendulums for clocks are particularly suited to mathematical workmen.

the ſubject is ſtill agreeable to me, being yet unincumbered with patents, the reaſon of which no doubt is, that the few wanted holds out no proſpect of gain.

Tubular pendulum.

Under theſe circumſtances, I offer you the following deſcription of my tubular pendulum, which I contrived about the middle of laſt July, and which I have juſt brought into action. The honourable mention which you made of my mercurial pendulum (Phil. Journal for May 1797) the excellent papers which you have at different times given to the public on this ſubject, and its importance to practical ſcience, eſpecially aſtronomy, leave me no room to doubt but this communication will be agreeable to you.

It acts on the principle of the gridiron.

It may here be proper to remark that the nature of this pendulum is by no means new; it is of the gridiron kind, and although extremely unlike it in outward appearance, is only a new ſpecies of that genus. Without further introduction, I will now proceed to give a brief deſcription, with reference to the accompanying ſketches, and then conclude with a few general remarks.

Deſcription of the tubular pendulum by the drawing.

Fig. 1. and 2, *Plate* XIII. drawn to a ſcale of one eighth of the real dimenſions, exhibit the ſhape of the whole inſtrument, in which the parts of action being completely concealed from view, it appears, excepting the uſual ſuſpenſion ſpring, to be made of ſolid braſs. The firſt of theſe figures gives a front view, the latter ſhews it as ſeen edgewiſe. This figure of the bob is uſed more on account of its being eaſy to make, and ſightly, than from any other conſiderations; it is made of one piece of braſs, about 7 inches diameter, 2,5 thick at the centre, and weighs about 15 pounds avoirdupoiſe: the front and back ſurfaces are ſpherical, with a thick edge or cylindrical part between them. The apparent rod is a tube of braſs, ſhewn in both figures to reach from the bob nearly to the top; this contains another tube and five wires in its belly, ſo diſpoſed as to produce altogether (like the nine-bar gridiron of Harriſon) three expanſions of ſteel downwards, and two of braſs upwards; whoſe lengths being inverſely proportioned to their dilatation, when properly combined, deſtroy the whole effect that either metal would have ſingly. The ſmall viſible part of the rod near the top, is a braſs tube, whoſe uſe is to cover the upper end of the middle wire, which is here ſingle, and otherwiſe unſupported.

5 Reckoning

Reckoning from the top the firſt action is downwards, and conſiſts of the ſpring, a ſhort wire 0,2 diameter, and a long wire 0,1 diameter; theſe all of ſteel firmly connected, reach down within an inch of the centre of the bob, and occupy the middle line of the whole apparatus. To the lower end of the middle branch, is faſtened the lower end of the interior braſs tube, 0,6 in diameter, which terminates a little ſhort of the top of the exterior tube, and produces the firſt dilatation upwards. From the top of the interior tube depend two wires 0,1 diameter, whoſe ſituation is in a line at right-angles to the ſwing of the pendulum, and reach ſomewhat lower than the attached tube itſelf, which they paſs through without touching, and effect the ſecond expanſion downwards. The ſecond action upwards is gained by the exterior tube, whoſe internal diameter juſt allows the interior tube to paſs freely through it: its bottom is connected with the lower ends of the laſt deſcribed wires. To complete the correction, a ſecond pair of wires of the ſame diameter as the former, and occupying a poſition at right angles to them, act downwards, reaching a little below the exterior tube, having alſo paſſed through the interior one without touching either. The lower ends of theſe wires are faſtened to a ſhort cylindrical piece of braſs, of the ſame diameter as the exterior tube, to which the bob is ſuſpended by its centre.

Deſcription of the tubular pendulum by the drawing.

Fig. 3. is a full ſize ſection of the rod, in which the three concentric circles are deſigned to repreſent the two tubes, and the rectangular poſition of the two pair of wires round the middle one, are ſhewn by the five ſmall circles. By copying this arrangement, from the elegant conſtruction of your own half ſeconds pendulum (Philſ. Journal for Auguſt, 1799) I avoided much trouble, which muſt have occurred to me, unleſs indeed, I had been impelled on the ſame idea, by the difficulty of contriving the five wires to act all in a row, with ſufficient freedom and in ſo ſmall a ſpace. *Fig.* 4. explains the part which cloſes the upper end of the interior tube: the two ſmall circles are the two wires which depend from it, and the three large circles ſhew the holes in it, through which the middle and other pair of wires paſs.

Fig. 5. is deſigned to explain the part which ſtops up the bottom of the interior tube, the ſmall circle in the centre is where the middle wire is faſtened to it, the others the holes for the other four wires to paſs through. *Fig.* 6. is the part which

 cloſes

Defcription of the tubular pendulum by the drawing.

clofes the upper end of the external tube, the large circle in the centre is the place where the brafs covering for the upper part of the middle wire is inferted, and the two fmall circles denote the faftening for the wires of the laft expanfion. *Fig.* 7 reprefents the bottom of the exterior tube, in which the fmall circles fhew the faftening places for the wires of the fecond expanfion, and the larger ones the holes for the other pair of wires to pafs through. *Fig.* 8. is a cylindrical piece of brafs, which fhews how the lower ends of the wires of the laft expanfion are faftened to it, and the hole in the middle is that whereby it is pinned to the centre of the bob. The faftening of the upper ends of the two pair of wires is done by fcrewing them into the pieces which ftop up the ends of the tubes, but at the lower ends, they are all fixed as reprefented in *Fig.* 8. I have only to add to this defcription, that the pieces reprefented by *Fig.* 7 and 8, have each a jointed motion, by means of which the fellow wires of each pair would be equally ftretched, although they were not exactly of the fame length.

In the apparatus thus connected, the middle wire will be ftretched by the weight of the whole; the interior tube will fupport at its top the whole except the middle wire: the fecond pair of wires will be ftretched by all except the middle wire and interior tube; the exterior tube fupports at its top the weight of the fecond pair of wires and the bob, and the fecond pair of wires are ftretched by the weight of the bob only.

The firft pendulum which I made of the tubular kind, had only three fteel wires, and one tube above the bob; that is two expanfions down and one up; and the quantity which one of brafs falls fhort to correct two of fteel, was compenfated for, by extending thofe branches of the rod below the bob, and bringing up an external tube to which the bob was affixed. There is an aukwardnefs in this conftruction, owing to the rod reaching about 13 inches below the lower edge of the bob, otherwife, it is not inferior to the one firft defcribed.

Difficulty of preventing flexure and ftarting motion in the gridiron,

The principles of the gridiron pendulum, I believe, have never been queftioned, indeed they cannot be; the difficulty of conftructing it ftrong enough to prevent lateral flexure in the lifting bars, and confequent friction in the holes of the different croffing pieces neceffary to bind it together, which occafion it to act by ftarts, has been the only obftacle to prevent its general application to the beft clocks.

To

To remedy this evil I contrived the one here treated of, and in my own judgment, have succeeded as near perfection as any effort of mine ever did. The tubes are drawn ſtraight and regular in an engine, in the manner that wire is done: and their ſtrength aſſure ſucceſs, preventing the poſſibility of any ſenſible bending, while the freedom of action prevents any irregular motion whatever. —is prevented by the uſe of tubes.

I have no farther quarreled with my own conſtruction of the mercurial pendulum, the principles of which are faultleſs, and form elegant; than that it is quite unfit for carriage, and an article too hazardous for me to deal in; but after all, candour obliges me to give the preference to the original one of Graham, which on account of ſome ſenſeleſs criticiſms about one part taking heat ſooner than another, evaporation of the mercury, &c. was too haſtily laid aſide. Graham undoubtedly made his pendulum before the expanſions of either braſs or mercury were accurately known; but an ingenious friend of mine, has from the beſt experiments, computed the different parts to mathematical preciſion, and introduced it a ſecond time to the world; and already, there are not wanting proofs of its practical excellence. Troughton's mercurial pendulum is perfect but not portable. Graham's pendulum approved.

Smeaton's pendulum with tubular bob is a good one, but on account of the glaſs rod, like my mercurial one, is totally unfit for tranſportation, and like Graham's, is unſightly; while all three labour under the unavoidable diſadvantage of not being applicable to the pyrometer in their compound and pendant ſtate. Smeaton's good; but not portable.

I am about to conſtruct a pyrometer fit to try the tubular pendulum in its finiſhed ſtate; a pin fixed in the centre of oſcillation, ſhall be contrived to act on a nice ſpirit level, and ſhew the expanſion, if any, in angular meaſure: by which I ſhall be enabled to adjuſt it; for I have no idea of adjuſting one by the going of a clock, that complicated apparatus will always, I fear, have too many errors of its own, to be uſed as a criterion of the merits of this more perfect inſtrument.

I will here ſubjoin the lengths of the different parts of tubular pendulums in inches; alſo, the expanſions of braſs and ſteel from which I computed them. The length of the interior tube is 31,9, and that of the exterior one 32,8, to which muſt be added 0,4 the quantity which in this pendulum, the centre of oſcillation is higher than the centre of the bob; theſe all Dimenſions and grounds of conſtruction of the tubular pendulum.

all of braſs. The parts which are of ſteel are the middle wire, including 0,6 the length of the ſuſpenſion ſpring, is 39,3, firſt pair of wires 32,5, and the ſecond pair 33,2. From experiments which I tried in the year 1794, I found my braſs to expand with 60° of heat 0,000640 on an inch, and my ſteel 0,0003966. Smeaton obtained under the ſame circumſtances 0,0006444 for braſs, and 0,0003833 for ſteel. Roy found braſs to expand 0,0006316 and ſteel 0,0003816.

Expanſions of braſs and ſteel.

The above lengths computed from my expanſions will I think be found very correct; if they are tried by Smeaton's, the pendulum will ſeem to err in exceſs, or gain with heat, about 0″,9 a day with 60° of Fahrenheit; if Roy's be uſed, it will err alſo in exceſs, under the ſame circumſtances ſomething more than one ſecond. The above ſtatement ſhews with what care experiments of this kind ought to be conducted. All the dimenſions are given in inches and decimals.

If my pyrometrical experiments turn out ſucceſsful, they may likely be the ſubject of another communication; and I will reſerve till then my reaſons for giving the preference to my own expanſions in the conſtruction of the inſtrument here deſcribed; for this letter, being twice as long as I firſt intended, ſhall now be concluded by,

Dear Sir,

Your obliged and obedient Servant,

EDWARD TROUGHTON.

November 3, 1804.

II.

Letter from W. Jessop, *Eſq. on an Improvement in the Proceſs of blaſting Rocks with Gun-powder.*

To Mr. NICHOLSON.

SIR,

If I am communicating to you what you already know, you will accept as an apology for it that it is not generally known, and will be intereſting to all who are concerned in mining or blaſting rocks with gunpowder.

Uſual proceſs of boring and blaſting.

The uſual proceſs, after drilling a hole and charging it with powder, is to introduce a wire or ſmall iron rod, to preſerve a com-

communication with the fuze, and then to ram up the remainder of the hole with ſtone pulverized by the operation of ramming it; after which the wire is withdrawn and the priming introduced.

This is a tedious operation, often attended with danger, and frequently the labour and powder is loſt by the priming hole being obſtructed in ſtriking out the wire with a hammer. is tedious, dangerous and uncertain.

I had been informed that inſtead of this tedious operation, the blaſting had been effected by introducing a ſtraw filled with fine powder, and then filling the hole with looſe ſand. Looſe ſand propoſed inſtead of ramming.

I hardly could believe this, conceiving that the part where there would be the leaſt reſiſtence would firſt yield to the exploſion, and therefore that the looſe ſand muſt of courſe be blown out.

But in the month of Auguſt laſt I tried the experiment in ſome very hard rock at Fortwilliam and it completely ſucceeded: I ſince then have tried it on the lime rocks at Briſtol with the ſame effect. Succeſs of the experiment.

A few days ſince, wiſhing to know how ſmall a quantity of ſand would produce the effect, I cauſed a hole of 1½ inch in diameter and 12 inches deep, to be bored in a knotty piece of oak of about 20 inches in diameter and charged it with three inches of powder, and upon that four inches of ſand, intending (as I ſuppoſed that muſt be blown out) to repeat the quantity of ſand inch by inch to the quantity that would be ſufficient, but to my great ſurprize, it ſplit the piece with great violence into ſix pieces. Oak 20 inches diameter ſplit in pieces under a charge of 3 inches ſand.

I then repeated it on a ſimilar log of oak, with a charge of two inches of powder, and *three* inches of ſand; it ſplit the piece in two, and ſent half of it into the air to the diſtance of forty yards. The ſame effect with leſs powder.

I think it may puzzle a philoſopher to account for this: I have ſuppoſed that the ſand in contact with the powder, firſt moving, the particles wedge each other faſt before the motion has time to be communicated to the whole. Conjecture as to the cauſe.

I have reaſon for thinking that a much leſſer quantity of powder will produce the effect in this way than by the common method; for as it lies looſe in its chamber and makes itſelf more room by a little yielding of the ſand, the whole will take fire, and eſpecially if when half the charge is put in, the ſtraw be introduced and then the other half charge, ſo that it may take fire in the middle of the charge. Leſs powder will probably be required in this way.

That

The fand appears to yield a little.

That the fand will yield a little, I think I may infer from having charged a tube of tin 14 inches in length and 1¼ diameter, with three inches of powder; on exploding it immerfed in water, I found *five inches* of the tube abruptly cut off, while the remainder was uninjured, but I do not confider this inference as decifive.

For want of time I was prevented from trying how much *lefs* than three inches of fand would have been fufficient.

I am, Sir,

Your moft obedient,

W. JESSOP.

London Coffee Houfe,
Nov. 8, 1804.

A charge of fand would probably deftroy artillery.

P. S. Might not this be ufeful on the evacuation of a fort, in deftroying cannon, inftead of the ineffectual mode by fpiking?

III.

On the Mucilaginous Matter of certain Vegetables, and their Ufe as a Subftitute for Gum Arabic: by Mr. THOMAS WILLIS; *being a Continuation of Experiments made upon the Subject by him, in Addition to thofe formerly publifhed in the Tranfactions of the Society of Arts.**

Letter to CHARLES TAYLOR, *Efq.*

SIR,

Mucilaginous powders from vegetable roots.

I HAVE taken the liberty of prefenting to the Society for the Encouragement of Arts, &c. fpecimens of the powder of vernal fquills, of white lily-roots, and of falop, for their confideration and trial. I believe the bulbs of the vernal fquill will be found to be equally ufeful as thofe of the hyacinthus non-fcriptus; and the ftrong mucilage from the white lily-root and the falop much more fo.

As gum-arabic has been at times, during the laft war, above 30*l.* per cwt. the multiplication of different fubftances that will anfwer the fame purpofe, will affuredly be an object of a very interefting nature to manufacturers in time of war.

* Extracted from their Tranfactions, Vol. XXI. The former experiments are in our Journal, VII. 30.

I have

I have ſent all the powder of white lily-root I had left; but am at the preſent time drying ſome taken from the garden two or three days ago. As ſoon as it is powdered, I will ſend you a large quantity of it.

I am, Sir,

Your moſt obedient Servant,

THOMAS WILLIS.

March 24, 1803.

SIR,

FROM the candour and approbation my paper on the uſe of the hyacinthus non-ſcriptus, as a ſubſtitute for gum-arabic, in ſome trials in calico-printing, met with from the Society for the Encouragement of Arts, &c. I reſolved to purſue the ſubject further; and now offer the following obſervations for your conſideration, in hope that trials may be made, that will be found to be of general benefit.

Having frequently obſerved, in old gardens, that the vernal ſquill grew very prolific, I conceived the root of them might be equally as mucilaginous as the roots of the blue bells. I procured three pounds ſix ounces of them on the 8th of July, 1802, and ſliced and dried them. They produced one pound one ounce of powder, one drachm of which was diſſolved in four ounces of water, by letting the mixture boil a minute or two. When the liquor was cold, there was a mucilage full as ſtrong as one made with a drachm of the powder of blue bells, in the ſame proportion of water; from which circumſtance, I ſhould think the vernal ſquill will anſwer the ſame purpoſes as the hyacinthus non-ſcriptus. If it ſhould, it is a root that may be very eaſily and plentifully propagated, and whenever gum-arabic is dear, may be found uſeful. It is to be remarked, that I found no pungency in the powder of the vernal ſquill. I have frequently eaten of it, and the taſte is rather agreeable. **Root of the vernal ſquill very mucilaginous.**

On the 18th of Auguſt, 1802, I collected four pounds of the white lily root, which yielded, when dried, rather more than one pound of powder. A drachm of it was diſſolved in four ounces of water, by gently boiling it a minute or two: the mucilage was much ſtronger than that made with the vernal ſquill, but ſomewhat darker coloured. This root may poſſibly **White lily root alſo very mucilaginous.**

poffibly anfwer the fame purpofes better than thofe above mentioned. The powder of the lily-root leaves a bitternefs on the tongue.

Gum obtained by folution and evaporation.

Twelve ounces of the frefh roots of the white lily being bruifed and preffed, yielded by evaporation one ounce and one quarter of brown gum.

There being a fmall quantity of it, I made no trial with it; but very probably the expreffed juice might be ufed by the calico-printers with advantage. Thefe roots can be procured at all times, and propagated to any extent; but it muft be obferved, that thefe bulbous roots are ftronger when they are without ftems, or only beginning to fhoot out leaves; and the prefent time is as proper as any that can be for procuring them.

Comfrey root.

I tried comfrey-root; but the dark colour of the cuticle of the root affected the folution, by making it of a dark dirty brown.

I do not pretend to claim any merit in making the above experiments. Every one who is acquainted with thefe roots, well knows that they are all mucilaginous. My defign is only your patronage, to render them ufeful in the arts, that their virtues and effects might not lay dormant, but be rendered ferviceable to trade.

Powder of falop root affords very ftrong mucilage; probably cheaper than gum-arabic.

After making the above trials, the powder of falop-root was ufed, by diffolving one drachm of it in four ounces of water, in the manner above mentioned. It produced a very ftrong mucilage, and, when cold, was a perfect jelly, and much clearer than either of the other folutions. I am greatly of opinion, that the powder of this root will not only anfwer all the purpofes of gum-arabic, but will be found full as cheap, if not cheaper, in proportion to its ftrength, than gum-arabic; but this is fubmitted to your confideration and trial.

I have fent fome fpecimens of the Powder of the Vernal Squill, of White Lily Root, and alfo of the Salop Powder, in order that they may be tried by your direction; and if they fhould be found of benefit to commerce, nothing would give me greater pleafure, than to find my flender abilities have been of utility to my country, and honoured with the approbation of the Society.

I am, Sir,

Your obedient fervant,

THOMAS WILLIS.

March 22, 1803.

Examination

IV.

Examination of Mr. Ezekiel Walker's Experiments and Theory of the Enlargement of the Horizontal Moon. In a Letter from C. L.

To Mr. NICHOLSON.

SIR,

YOUR correfpondent Mr. Ezekiel Walker, has favoured the world with a new attempt to account for the apparent enlargement of the horizontal moon *, in which, after indulging his wit, and expreffing his aftonifhment at the expence of thofe who have laboured on the fame fubject, he proceeds to affure us that the *pictures on the retina are not permanent but vary as the dimenfions of the pupil vary.* Affertion of Mr. Walker that optical images vary with the pupil.

That you, Mr. Nicholfon, who are in fome refpect accountable to the world for what you admit into your Journal, and who poffefs a juft celebrity for the manner in which you have executed the tafk,—that you fhould have omitted to favour the writer and your readers fo far as to reject his paper, is to me no otherwife to be explained than by referring it to your impartiality; which may have induced you to leave the refutation of alledged facts to your correfpondents rather than enter into a difcuffion refpecting them yourfelf.

I cannot for a moment fuppofe you to have had a doubt concerning this imaginary novelty in optics. If Mr. Walker's pofition were true, the magnifying powers of the fame telefcope would vary with its aperture; a well illuminated theatre or room would become contracted in its apparent dimenfions; the paper at which I now look would fuddenly become larger when I cover the candle with my hand; and in a word we fhould have no certainty if the magnitudes of things were to appear different with every change of the pupil. Confequences that would follow if it were well founded.

None of thefe things do in fact happen, and it feems almoft trifling to infift upon them. Still lefs is it neceffary to have recourfe to lines and figures: but for the fake of your lefs inftructed readers I will meet Mr. Walker by correct experiments of the fame defcription as his own. Experiments of Mr. Walker repeated.

* Philof. Journal, IX. 164.

Exp.

The image formed by a lens with a broad aperture is the fame as

Exp. 1. I took a clear double convex lens fupported on a ftand, and placed the fame oppofite an Argand's lamp; fo that a good image of the flame was formed upon a paper fixed againft the wall of the room. From the flame to the lens the diftance was $49\frac{1}{2}$ inches and from the lens to the image it was 88 inches. The horizontal breadth of the image was repeatedly meafured = 1.58 inches and to a greater degree of precifion than one hundredth of an inch.

with a contracted aperture.

Exp. 2. The lamp and lens were fuffered to remain unaltered in the fituations they poffeffed in the former experiment; but a fcreen was placed clofe to the lens, having a circular hole of one inch diameter concentric with the lens itfelf. The image was then fainter but very diftinct, and meafured exactly the fame as before.

In both thefe experiments the papers on the wall were feveral times changed to prevent any deception from the appearance of former meafures; and the meafures were taken along the fame horizontal line by the help of a line on the wall.

Hence it was feen that a diminution of the pupil or aperture, to admit lefs than one thirty-eighth part of the light, made no difference in the image.

The experiments repeated with an achromatic lens.

Exp. 3. I took a good achromatic object glafs of Dollond, having an aperture of 1.9 inch and placed it at the diftance of $52\frac{1}{2}$ inches from the lamp; when it gave a good image on the wall at the diftance of $70\frac{1}{4}$ inches from the lens. The horizontal breadth of the image was 1.2 inches.

Exp. 4. When the experiment was repeated with no other change than that a fcreen with a central hole of 0.3 inches was placed before the lens, the image proved 1.2 inches as before.

The two laft experiments were made with the fame care and precautions as the former. They fhew that no diminution of the image takes place when the pupil is diminifhed to admit lefs than one fortieth part of the light.

Probability that Mr. W. was deceived, and how.

I may not conclude, Sir, without making fome obfervations on the direct contradiction of my experimental refults to thofe of Mr. Walker's. May I afcribe to him any thing more than inaccuracy? Affuredly I do not. Neverthelefs, if he meafured fo uncertain a thing as *the length* of the flame of a candle without being affured by repeated experiments that its permanency was entitled to fome confidence; if he held his fcreen nearer

nearer to his lens when the light was rendered fainter by his card; if he miftook the greater vifibility of the fmaller image for a proof that it was in the focus of the lens; in a word, if he did not paufe and examine former facts before he adopted a conclufion fo repugnant to many of them, I would fubmit to yourfelf and readers whether he has acted confiftently with the rules of philofophical inveftigation, or has any reafon to prize his own folution beyond that which is founded on the rules of linear perfpective*.

I am, Sir,

Your humble fervant,

C. L.

V.

The Method of preparing Chinefe Soy. By MICHAEL DE GRUBBENS.†

IN the acts of the Swedifh Academy, for 1764, page 38, we find a defcription of the preparation of Chinefe Soy, by the late Captain Ekeberg; but as this defcription is not only incomplete but even deficient in accuracy, fo as not to produce the true Chinefe compound, I am well convinced that Mr. Ekeberg muft have been unacquainted with the actual procefs. It is moft likely that he depended upon information given him by the Chinefe who are not always to be trufted; a fact of which I have had fufficient reafons to be convinced, during

Ekeberg's account of foy is erroneous.

* Mr. W. having fuppofed that the want of land objects muft take away the notion of diftance at fea, reminds me of an incident concerning the Panorama of Black-Friar's Bridge. This picture was exhibited with indications of a confiderable wind with waves on the river, at the fame time that the buildings on the London fide were feen reflected in the water. I took notice of this inconfiftency, and the ingenious author replied that the water had been at firft painted as if fmooth; but it was found neceffary to put in the waves in order to give diftance by their regular diminution in perfpective, and that the reflections were kept in from a notion that they would rather add to the effect than offend by their want of perfect truth.

† Extracted from the Memoirs of the Academy of Sciences of Stockholm for 1803, by M. Linborn, and inferted in the Annales de Chimie, Vol. L. from which the prefent tranflation is made.

my

my five years refidence in their country, in my attempts to afcertain correctly the manner of treating a fpecies of filk-worm which affords its thread five or fix times in the year, as well as their methods of dying cotton and filk and feveral other branches of domeftic œconomy.

Having fince received for a confiderable remuneration true notions concerning thefe objects, I faw how far the former recitals had differed from the truth. The fame thing happened in my attempts to difcover the preparation of foy; but as I at laft became perfectly acquainted with it, I think it proper to communicate the fame to the academy.

Ingredients of foy.

Soy is prepared with a fpecies of haricots (which are white and fmaller than thofe of Turkey) wheat flour, common falt, and water. The following are the proportions; 50 pounds of haricots, 50 pounds of falt, 60 pounds of wheat flour, and 250 pounds of water.

Procefs. Hericots are boiled in water,

After having well wafhed the haricots, they are boiled in well-water in an open veffel for fome hours, or until they have become fo foft as to be worked between the fingers. During the boiling they muft be kept covered with water to prevent their burning, and care muft be taken not to boil them too much, becaufe in that cafe too much of their fubftance would remain in the water of decoction. The haricots being thus boiled, are taken out, and put into large fhallow wooden veffels, which in China are made of thin ftaves of bamboo, two inches and a half in depth, and five feet in diameter. In thefe they are fpread out to the depth of two inches, and when they are cold enough to be worked with the hand, the wheat flour is gradually thrown in and well mixed with the haricots, until the whole of the before-mentioned quantity has been ufed. When the mafs becomes too dry, fo that the flour does not mix well with the haricots, a little of the hot water of decoction is added.

—then mixed with wheat flour,

—and thinly fpread out in a veffel covered up.

The whole being well mixed, the mafs is then fpread abroad in the veffels before-mentioned, taking care that its depth fhall not be more than an inch or an inch and a half; and the mafs is then covered by a lid which fits exactly. When the mafs begins to become mouldy and heat is difengaged, which happens after two or three days, the cover is then raifed by putting two fticks beneath it, in order that the air may have free accefs. During this time a rancid odour exhales, and if the mafs

A peculiar mouldinefs is fuffered to come on by admitting the air:

mafs becomes green, it is a fign that the whole goes on properly; but if it begins to be black, which muft be carefully noticed, the lid muft be elevated ftill more, in order that the mafs may have more air. If it once becomes black, the whole is fpoiled.

As foon as all the furface is covered with green mouldinefs, which ufually happens in eight or ten days, the cover is taken off, and the compound is expofed to the fun, and the air for feveral days. When it has become as hard as a ftone, it is cut into fmall fragments, which are thrown into an earthen veffel, upon which the 250lbs. of water, having the 50lbs. of falt firft diffolved in it are poured. The whole is then well ftirred together, and notice is taken of the height at which the water ftands. If it be not convenient to put all the mixture into one veffel, a number may be ufed, taking care that the materials be proportionally diftributed in each. The veffel thus filled is placed in the fun, and its contents ftirred up regularly every morning and evening, and a cover is put on at night to defend it from the cold, as well as to prevent any rain from finding entrance either by day or night. The hotter the fun the fooner will the foy be completed. The procefs is feldom undertaken but in the fummer, notwithftanding which it lafts two or three months.

—the compound is then dried,

cut in pieces and mixed with falt water:

and in a veffel placed in the fun and daily agitated

for two or three months.

As the mafs diminifhes by evaporation, well-water is added; and this digeftion is continued till the falt water has entirely diffolved the flour and the haricots. The veffel is ftill left for fome days in the fun, in order to compleat the folution ftill more effectually, as the good quality of the foy depends upon this circumftance; and the daily ftirring or agitation is continued to the very laft.

When the fluid has become fmooth and homogeneous

When at length the mafs has become very fucculent and oily, the whole, as well the thick as the more fluid portions, is poured into bags, through which the foy is preffed, and is then clear and ready for ufe. It is not afterwards boiled, as Mr. Ekeberg pretends. It is to be kept in bottles well corked. The Chinefe who deal in this article keep it in large pitchers well clofed. Before it is ftrained in the prefs the foy is of a deep brown colour, but it afterwards becomes black.

it is ftrained by preffure and is foy.

The Chinefe alfo prepare two kinds of foy from the dregs which remain. The firft time they add 150lbs. of water and

Inferior foy from the dregs.

30lbs.

30lbs. of ſalt, and after having preſſed the maſs, they again add 100lbs. of water and 20lbs. of ſalt, always proceeding as before deſcribed.

The two laſt kinds of ſoy are not ſtrong, but very ſalt, more eſpecially the latter, which is alſo lighter coloured. Theſe two kinds are the moſt common in China, and are uſed both by natives and Europeans. The differences of price are as 8.4.1.

This proceſs has been often verified,

In this manner I prepared in 1759, at my reſidence in Canton, the whole of the ſoy I made uſe of, and I alſo brought ſeveral bottles with me to Sweden. It was ſucculent, oily, moderately ſalt, and very different from what is commonly ſold in Europe. With regard to its taſte, it might be put in competition with that of Japan, which is generally conſidered as the beſt.

by the writer himſelf.

This deſcription is more particularly to be depended upon, as I always made the preparation myſelf; and I can venture to aſſert that this proceſs is the only one for obtaining ſoy of the beſt quality.

Soy contains no ſpice or ſugar, and is ſold in China for three half-pence a pound.

Mr. Ekeberg affirms that the ſoy is boiled with the addition of ſugar, ginger and other ſpices; but this is without foundation, and cannot be true, for a lb. Chineſe of ſoy coſts no more than two candareens of Chineſe money *. This was the common price during my whole reſidence at China, and is too low to admit of theſe ingredients in the preparation. It is alſo the fact that ſoy has no indication either of ſugar or ſpice in its taſte; its principal character is ſaline.

VI.

On the Laws of Galvaniſm. In Letters from C. WILKINSON *and* THOMAS HARRISON, *Eſqrs.*

Windermere Lake, Weſtmoreland, Nov. 8.

DEAR SIR,

Some account of Mr. Gough.

I TAKE the opportunity of a few days relaxation in this romantic and pictureſque ſituation, to trouble you with ſome

* The Chineſe pound or tael is, I think, about 20 ounces, and the candareen is a little more than three farthings, that is to ſay, it is one tenth part of the mace which is valued at eight-pence. N.

galvanic

galvanic obſervations, which principally originate from a very pleaſureable converſation I have lately had with my friend Mr. J. Gough, of Kendal. To you, Sir, this gentleman, as a philoſopher and mathematician, is well known. Although deprived of ſight in very early infancy, it is amazing to obſerve the rapidity with which he proceeds in geometrical inveſtigations. Independent of this very ſtriking acquiſition, he is the beſt botaniſt and natural hiſtorian this country poſſeſſes. To theſe branches of knowledge he adds very conſiderable claſſical knowledge; and in his pneumatic enquiries he places no dependance upon any of the gaſes which he does not prepare himſelf. To converſe with ſuch a perſon is to be improved. To ſome lectures I delivered on galvaniſm a few days ago, at Kendal, I was honoured with his attendance; and in conſequence of the law I was attempting to explain, relative to the powers of galvaniſm on metallic bodies, I was favoured, the ſubſequent day, with the following letter from a very intelligent and well-informed profeſſional gentleman at Kendal, Mr. Harriſon, brother-in-law to Mr. Gough; the particulars of which I dare ſay you will find ſufficiently intereſting to merit inſertion in your valuable Journal.

To C. WILKINSON.

DEAR SIR, *Kendal, Nov, 5,* 1804.

I MENTIONED to you yeſterday my doubts reſpecting the law of galvaniſm, which you have laid down in Nicholſon's Journal for March 1, 1804; where you ſay, that the igniting powers in batteries of the ſame total ſurface, are as the ſquares of the ſurfaces of the elementary plates, ſingly taken in each: This, if I miſtake not, you repeated in your lecture of laſt night; at which time I expreſſed my doubts to Mr. Gough, and we afterwards examined the data given in the paper before-mentioned; on which we made the following calculations, and found that your concluſion was ſtrictly true according to your data, but that the forces of different batteries expoſing unequal ſurfaces, will be in the ratio compounded of the number of plates in each battery, and the ſixth power of the ſides of the elementary plates ſingly taken in each. The data you have given are the following: 100 plates of four in. ſq. ignited

Inveſtigation of the power of different piles in burning wire.

Inveſtigation of the power of different piles in burning wire.

nited half an inch of wire; 400 plates ignited two inches; and 50 plates of eight inch. ſq. ignited 16 inches of the ſame wire.

Now in order to accommodate theſe numbers to algebraic calculation, we will ſuppoſe a compariſon of three batteries, two of which are compoſed of plates of equal diameters, but differing in number; the third ſhall alſo differ from the firſt and ſecond in the magnitude of its plates, and agree with the ſecond in the number of them.

Let n be the number of plates in the firſt, m the number in the ſecond and third batteries; w and y the diameters of the two ſets of plates. Then, if the power of any ſingle plate bear any conſtant ratio to its ſurface, it will alſo have a conſtant ratio to ſome power of its diameter, becauſe the ſurface is as the ſquare of the diameter in ſimilar figures: let p be that power; let l, k, and z expreſs the lengths of wire which are ignited by the 1ſt, 2d, and 3d batteries: then from the firſt datum, $n : m :: l : k$, becauſe the effect is as the number of equal plates, according to experiment. Again, ſeeing that w is the diameter of the plates in the firſt and ſecond batteries, and y the diameter of thoſe compoſing the third, and p expreſſes the power determining the law of force for plates of different diameters, the force of a plate in the firſt and ſecond batteries, will be to the force of a plate in the third, as $w^p : y^p$, and as the number of plates in the ſecond and third is equal, we ſhall have $m w^p : m y^p :: k : z$, becauſe as $\overset{\text{Plate.}}{1} : \overset{\text{Plates.}}{m} :: w^p : k$, and as $\overset{\text{Plate.}}{1} : \overset{\text{Plates.}}{m} :: y^p : z$, then as $w^p : y^p :: k : z$, but as $n : m :: l : k$; therefore $n w^p : m y^p :: lk : kz :: l : z = \frac{l m y^p}{n w^p}$.

To apply this general theorem in order to determine the value of p in all caſes, we will begin with your own experiment made with equal ſurfaces on different numbers.

Since 100 plates, of four inches, ignite half an inch of wire, and 50 plates, of eight inches ſq. ignite 16 inches, we have $n = 100$, $m = 50$, $w = 4$, $y = 8$, $l = \frac{1}{2}$, $z = 16$; therefore $100 \times 4^p : 50 \times 8^p :: \frac{1}{2} : 16$; but ſince $8 = 2 \times 4$, $8^p = 2^p \times 4^p$, therefore $2 : 2^p :: \frac{1}{2} : 16$, and $16 = \frac{2^p}{2^2} = 2^{p-2}$;

but

but $16 = 2^4$; hence $2^4 = 2^{p-2}$; therefore $p = 6$; hence the force of plates are as the 6th power of their diameters.

When the ſurfaces are equal, the igniting force is as the 4th power of the ſides of the individual plates; for in this caſe we have $nw^2 = my^2$, and $n : m :: y^2 : w^2$; therefore the theorem $nw^6 : my^6 :: l : z$, becomes $y^2 w^6 : w^2 y^6 :: l : z$; that is, $w^4 : y^4 :: l : z$, which is the law in your particular caſe.

This theorem will be found uſeful in eſtimating the force of batteries in general; as an example of which we ſhall calculate, by means of it, the force of your intended battery, conſiſting of 50 plates, each 24 inches diameter. In order to determine the quantity of wire that will be ignited by ſuch a battery, we have $n = 100$, $m = 50$, $w = 4$, $y = 24$, or ſix times w, $l = \frac{1}{2}$; therefore $100 \times 4^6 : 50 \times 6^6 w^6 :: \frac{1}{2} : z$; that is, $2 : 6^6 :: \frac{1}{2} : z$, and $z = \frac{6^6}{4} = 36^2 \times 9$ inches, or 972 feet.

I am, Dear Sir,

Your's reſpectfully,

THOMAS HARRISON.

Stramongate, Kendal.

YOU will obſerve, Mr. Editor, from the above theorem, that my ſtatement of the powers of two different galvanic arrangements, equal in their metallic ſurfaces, being as the ſquares of the ſurface of two individual plates, or, which is the ſame, as the 4th power of the diameter of each plate, is correct only where the metallic ſurfaces are equal; when they are not equal, then the powers are as the 6th power of the diameters, or as the cubes of their reſpective ſurfaces.

Experiments on the decompoſition of water.

In a ſhort time you will have the pleaſure of a paper from Mr. Gough, relative to the decompoſition of water by galvaniſm: With this view we tried together the following experiments, the reſults of which tend to favour the explanation I have attempted in the Elements I ſometime ſince publiſhed.

For this purpoſe I employed 270 plates, containing a ſurface of 6720 ſquare inches, and connected with the poſitive and negative ſides by wires of platinum. Theſe platinum wires were inſerted in a diſh of water at eight inches from each other; the poſitive wire diſengaged oxigen gas, the negative

wire hidrogen gas. The decompofition took place very flowly; in proportion as the wires were approximated, the decompofition increafed in the fame ratio.

The platinum wires were then inferted into a capillary tube whofe bore is about $\frac{1}{20}$ of an inch, filled with water; no fenfible effect was produced till the wires were placed at the diftance of half an inch, and then only a folitary bubble became difengaged from the negative wire, never completely detached, but hovering round the extremity, not being fet at liberty by the evolution of another particle.

When the platinum wires were inferted in a tube whofe bore is $\frac{1}{8}$ of an inch, at the diftance of fix inches a flight decompofition was obferved, and which neceffarily increafed in proportion as the wires approached each other.

When the platinum wires were inferted in different glaffes of water, and the extremities of a frog deprived of its integument conftituted the connecting medium between the two glaffes, or a wet piece of cloth, at the diftance of three inches a flight decompofition was obferved.

When two wires are connected with the different ends of a galvanic battery, the wires immediately participate, as to their electrical intenfity, with the refpective plates with which they are in contact.

Inveftigation of the laws of decompofition of water by galvanifm.

Let us fuppofe P the wire from the pofitive end of the battery, and N the negative. *Pl. XVI. Fig.* 2. Let us fuppofe that the galvanic arrangement is fuch, that, when the wires are placed at the diftance *a, a,* the decompofition commences. If the wires be placed at *b* and *b,* the decompofition is confiderably increafed; and this increafe will go on, till the maximum will be at *d, d,* when the wires are infinitely near each other without being in contact.

The pofitive wire, according to its electrical intenfity, produces a correfponding ftate in all fluids with which it is in contact, proportionate to their capacities; fo alfo the negative wire; and it is evident from the principle of electrical charges, that the influence of each muft extend to equal diftances; it will therefore follow, that when at *a, a,* fuppofing thefe points the apices of two equal cones, the areas of their refpective bafes will be the leaft poffible, and confequently, in their concourfe at *i,* will only influence a fingle particle of water. When the wires are placed at *b, b,* then *c i* will exprefs

prefs the radius of the circular bafe (fuppofing $b\,c = a\,i$), and confequently a greater proportion of water fubjected to the influence; and therefore evidently when at *d*, *d*, or infinitely near, the maximum of decompofing effects muft take place. This we obferve is confonant to experiments.

Inveftigation of the laws of decompofition of water by galvanifm.

If a particle of water confifts of a particle of oxigen and of hidrogen in a ftate of chemical union, and if an agent be employed fufficiently fubtle to penetrate the conftituent particle of water, a feparation will take place. Thus caloric will deftroy the aggregation of the conftituent particles of many fubftances, without producing any decompofition; in this cafe it only acts by penetrating the interftices formed by the conftituent particles, without pervading the fubftance of the conftituent particle itfelf. Thus the portions of water, although participating of the refpective electrical intenfities of the two wires, as yet undergo no decompofition; as yet it has not completely pervaded the particle of water. When a particle of water is influenced on one fide by the pofitive wire, and on its other fide by the negative, then the particle of water becomes a fmall Leyden phial; and if the energy of the apparatus fhould be adequate to overcome the refiftance between the two fides of the particle of water, a feparation is produced, and the water becomes refolved into oxigen and hidrogen.

This deftruction of the water will take place at *i*, the intermediate diftance between *a*, *a*; becaufe, when thus remotely fituated, only a fingle particle of water becomes thus operated upon; when at *b*, *b*, a greater number of particles of water become thus influenced, and confequently the decompofition is more rapid.

If water placed in a capillary tube be employed, it is evident, however near the platinum wires are approximated, the areas of the bafes of the refpective cones will be not at all increafed, and, confequently, the maximum of its power will be confined comparatively to a folitary particle of water. The inftant a particle of water becomes thus feparated into its elementary portions, it might be expected that thefe portions would be evolved at *i*, as being the point of concourfe between the pofitive and negative ftates; but in this form they are not in the gafeous ftate; they require caloric, &c.; and that they do not very readily acquire this other principle

neceffary

neceſſary to conſtitute them gaſes, is evident from the oxigen, if an oxidable metal be employed at the poſitive end, entering into combination with the metal, inſtead of being diſengaged in an aeriform ſtate.

Water is a conductor of electricity; its capacity for this principle being in proportion to its conducting powers. When converted into vapour, its conducting powers are increaſed, and ſo alſo in the ſame proportion its capacity, as is evident from water, when undergoing this change, abſtracting electricity from ſurrounding bodies.

When a particle of water is reſolved into its elementary portions, a change takes place in their capacities for electricity, caloric, &c. at the very naſcent inſtant. Oxigen we know, from other experiments, has its capacity for electricity increaſed, and muſt acquire this principle before it can aſſume a gaſeous form. This acquiſition will be effected with the greateſt readineſs in the direction towards the poſitive wire; while the hydrogen particle having its capacity diminiſhed, will be ſimilarly determined towards the negative wire.

For a future Journal I will trouble you with ſome experiments relative to this change of capacity. I am anxious firſt to peruſe Mr. Gough's intended paper, from whoſe philoſophical abilities much may be expected.

Having extended this paper to a greater length than I at firſt expected, I ſhall defer, till another opportunity, a reply to Mr. Cuthbertſon's obſervations, and flatter myſelf I ſhall be enabled to clearly point out the error into which Mr. C. has fallen.

Cure of rheumatiſm and palſy by galvaniſm.

Although your Journal may not be deemed the proper medium for any medical communications, as I have lately witneſſed the very powerful effects of galvaniſm in rheumatic and paralytic affections. Whatever may prove of benefit to mankind will, I am perſuaded, be readily admitted into your valuable Work.

I am, Dear Sir,

Your's, with the greateſt reſpect,

C. WILKINSON,

Surgeon, 19, *Soho Square, London.*

Remark

VII.

Remark upon an Aſſertion of LAVOISIER, *which has been repeated by eminent Chemiſts. By* P. PREVOST, *Profeſſor at Geneva, and Correſpondent with the National Inſtitute of France.* *

Aſſertion of Lavoiſier, that ſince bodies are contractible by cold, their particles are not in contact.

"IT is a conſtant phenomenon in nature . . . . . that when any body whatever is heated . . . . . its dimenſions become increaſed in every direction . . . . . If after having heated a ſolid body to a certain point, and by that means ſeparated its particles more and more, it be then ſuffered to cool, theſe particles will approach each other, according to the ſame gradation by which they were before ſeparated; the body will paſs through the ſame dimenſions it before poſſeſſed; and if it be reduced to the ſame temperature as at the beginning of the experiment, it will reſume its original volume. But as we are very far from being able to produce an abſolute ſtate of cold; but on the contrary, as there is no degree of refrigeration which we cannot ſuppoſe to be capable of being further augmented, it muſt follow that we have not yet ſucceeded in bringing the particles of any body as cloſely together as is poſſible, and conſequently that *the particles of no body in nature can be in a ſtate of contact;* a very ſingular concluſion, but to which nevertheleſs it is impoſſible to refuſe our aſſent."—*Elementary Treatiſe of Chemiſtry, by Lavoiſier, Vol. I. at the commencement.*

The ſame aſſertion repeated,

The ſame aſſertion is repeated ſeveral pages further, where an example is taken of a veſſel filled with ſmall leaden ſhot. "The balls touch each other," ſays our author, "whereas *the particles of bodies do not touch,* but are always kept at a ſmall diſtance from each other by the effort of caloric."

The authority which this celebrated chemiſt enjoys in the learned world, is probably the only cauſe why this concluſion has been adopted. It is at leaſt certain that it is not legitimate, though it has been ſo often repeated; and more particularly in a late work, no leſs remarkable for the depth of reaſoning and extent of knowledge which it diſplays. Ber-

* Annales de Chimie, Vol. L. p. 58.

thollet,

by Berthollet. thollet, in his *Effai de Static Chimique,* Tom. I. p. 24, parag. 2,* has thefe words: "Cohefion is the effect of that affinity which the particles exercife on each other, and keeps them at a diftance, determined by the equilibrium of this force with thofe which are oppofed to it; for the property poffeffed by the moft compact bodies of undergoing a diminution of volume by reducing their temperature, proves that there is no immediate contact between their parts."

Apology for Berthollet, After all, it is not furprifing that an author, who has undertaken and fuccefsfully executed a work of fuch immenfity with regard to chemiftry, fhould refer to one of his moft celebrated predeceffors for the folidity of an argument which relates to the principles of natural philofphy; and this conduct is fo much the more natural, as the argument never having been contefted, though frequently quoted, a confiderable prefumption arifes in its favour, to difpenfe with any careful examination in cafes where it is not intended to be farther applied. Now it does not appear that any farther ufe is made of the propofition throughout the great work to which our attention is now directed. I therefore confider the paffage here extracted, rather as the occafional mention of a fingular paradox, than as a thefis which the author was defirous of eftablifhing.

whofe authority may neverthelefs affift in eftablifhing this error. Let this be as it may, it will notwithftanding follow, that the mere incidental mention of an affertion by an author fo juftly refpected, will be confidered as a proof of its truth; and that the numerous difciples of this great mafter will repeat it with confidence. It is therefore defirable that its want of foundation fhould be infifted on, and that it fhould be directly refuted in a Journal of extenfive circulation; more particularly amongft thofe who are moft likely to be mifled by fuch an error.

The author does not difcufs the propofition itfelf, but only infifts that expanfion and contraction prove nothing. Before we proceed, it will be proper to make an effential diftinction. The affertion is as follows: *The particles of bodies do not touch each other;* or in other terms, *there is no immediate contact between the elementary parts of bodies.* Now, to fpeak with propriety, I do not at all intend to deny this affertion, neither do I propofe to eftablifh it. All that I intend to do is, to fhew that it is not legitimately concluded

* See Lambert's Tranflation, Vol. I, p. 2.

from

from the principle whence it has been deduced; namely, the diminution of volume which accompanies depreſſions of temperature. Now I ſay that this diminution does not at all prove that the elements of bodies have no immediate contact, and conſequently that this laſt aſſertion is ſo far gratuitous. And this is the whole of what I deſign to prove.

We can eaſily conceive expanſion without ceſſation of contact.

If it be true that there are numerous conceptions and examples which are evident and eaſy to be given, of bodies ſubject to dilation and condenſation without their parts ceaſing to be in immediate contact, it muſt neceſſarily be concluded, that dilation and condenſation can afford no proof in this reſpect. For why ſhould we refuſe to admit ſome of theſe conceptions, or to apply ſome of theſe examples to the caſe of the elements?

1. Inſtance: the legs of compaſſes may open and ſhut.

1. Conceive the particles to be elongated and united by their extremities, like the legs of a pair of compaſſes; and they may turn with regard to this point of union, as a centre, and produce condenſations and dilations of the whole apparent maſs of the body ſucceſſively.

2. Sponge, &c. may be compreſſed or enlarged.

2. A dry ſponge, or fruit, or mucoſity, being plunged in water, will dilate without any ceſſation of the ſenſible contact; and on the contrary, the ſame bodies, when wetted, if expoſed to become dry, will undergo condenſation.

3. Expanſion in cryſtals.

3. The example of the dilatation of ice and ſome metals by cryſtallization, an example formally remarked, ſtudied, and well explained, particularly by the latter of the two learned chemiſts I have cited, muſt ſhew the falſity of the aſſertion which I refute.

Upon reading the work of Berthollet, and taking notes of ſuch things as I was deſirous of retaining, or concerning which I ſaw reaſon to doubt, I wrote what is here tranſcribed, *i. e.* the ſummary indication of the three conceptions, or examples, proper to elucidate this ſubject.

Similar remarks by Leſage.

I had no intention of ſubmitting my remark to its natural judges, when five or ſix weeks afterwards, being employed in a very different occupation, I found in the papers of a learned * philoſopher, which were entruſted to me at his death,

* George Louis Leſage, Correſpondent of the Academy of Sciences of Paris, afterwards Correſpondent of the National Inſtitute, Member

death, a card which contained the ſame remark, but in a more abridged form, and by ſimple indication. As it offers a variety of examples and conceptions of the ſame nature as thoſe which I have given, though nevertheleſs different, I ſhall here give them verbatim :

Note of G. L. Leſage.[1]

" Methods of ſhewing that this conſequence does not follow :

Expanſion and contraction, 1. Of the hands; " 1. Four fingers of one hand introduced more or leſs between the four intervals of the fingers of the other hand.

2. Combs, &c. " 2. The ſame with a couple of bruſhes, or cards, or combs.

3. Cotton, &c. " 3. Carded cotton, or hair, and ſoap lather.

4. Snow. " 4. Snow.

5. Oſcillation. " 5. Agitation or oſcillation, which renders the ſame particles alternately contiguous and ſeparate more than a thouſand times per minute."

Concluſion. The peruſal of this note determined me to publiſh mine; not only becauſe I found myſelf authorized by this coincidence, but alſo to render a firſt homage to the memory of a philoſopher no leſs modeſt than ingenious, and to begin in ſome reſpect to execute his will, by publiſhing at leaſt one of his notes without making any change. The laſt conception offered in this laſt ſhort note, will probably induce ſome philoſophers to reflect again upon this ſubject, and may perhaps lead them towards the opinions of the writer.

[1] Member of the Royal Society of London, and of ſeveral other learned Societies, died at Geneva, the 28th Brumaire, in the year XII. (Nov. 19, 1803). His principal writings have not yet been publiſhed. I ſhall ſoon give a ſhort notice, as well as ſome details of his literary life, proper to facilitate the peruſal of his works, and eſtabliſh their originality. P.

Account

VIII.

Account of a Memoir on chamoying of Leather. By M. SEGUIN. *

M. SEGUIN, who has already publiſhed ſeveral intereſting works on the arts, relating to the preparation of ſkins, has lately read to the Inſtitute a firſt memoir on chamoying, from which we ſhall give an extract.

Mechanical operations of chamoying.

The author ſtates, that the art of chamoying conſiſts in diſpoſing the ſkins to receive the oil; in impregnating them with it by different operations, of which he probably reſerves the details for a ſecond memoir; in then cauſing them to undergo a ſpecies of fermentation; in expoſing them to the air; and, laſtly, in taking from them, by means of potaſh, the exceſs of oil which is uſeleſs to them.

He afterwards paſſes to the chemical examination of a chamoyed ſkin.

Chemical examination of chamoyed leather.

He found that this ſkin did not undergo any alteration by a long boiling in water; but that, if any acid whatever is added (M. Seguin made uſe of ſulphuric acid), the ſkin diſappeared entirely; that a certain quantity of a concrete oil ſwam on the ſurface of the liquid; that the liquor contained gelatine; and that, by its evaporation, it depoſited cryſtals of ſulphate of potaſh. He alſo aſcertained, that on pouring gelatine into a ſolution of ſoap, an inſoluble precipitate is obtained, which, treated with an acid, comports itſelf exactly like a chamoyed ſkin.

Gelatine poured into a ſolution of ſoap, forms a precipitate which reſembles a chamoyed ſkin in its chemical properties.

Theory of the operation.

Theſe reſults, and the conſiderations to which the expoſition of the principal operations of chamoying have given riſe, induced M. Seguin to conclude that the ſkins, in the fermentation which they undergo, yield a part of their oxigen to the oil; that the potaſh employed to cleanſe them, forms a ſoap with the oxigenated oil; that one part of this ſoap, by combining with the diſoxigenated ſkin, produces the inſoluble ſubſtance which makes the ſkin chamoyed; and that the other part ſerves to conſtitute that greaſe which remains in the leather.

* From Bulletin des Sciences, Tom. III. p. 209.

This

This work is the more interefting, becaufe hitherto no one has confidered chamoying in a chemical view, and becaufe the analyfis to which M. Seguin has fubmitted his refults, renders it fufceptible of acquiring that perfection which the difcoveries of this chemift have already given to the art of the tanner.

IX.

Analyfis and Decompofition of a Liquor employed to render Stuffs impermeable to Water. By M. Vauquelin. *

It is known that, for fome years, different perfons have been fuccefsfully employed in rendering ftuffs impermeable to water, an object of great importance in the clothing of foldiers and failors.

The inventors of this procefs have hitherto kept the means they make ufe of fecret: there was only reafon to fuppofe that fome fat oil was the bafis of their receipts, but experiment has not yet developed it.

A bottle of this preparation, of which the efficacy was known, having accidentally come into my hands, gave me a defire to inveftigate its compofition; but before ftating the plan which I followed with this view, I fhall defcribe its phyfical properties.

Phyfical characters of a liquor for rendering ftuffs impermeable to water.

It is a white liquor, milky and opake, of a bitter tafte, and fmelling like foap; on its furface there is a fpecies of cream refembling that of milk, and it ftrongly reddens the tincture of turnfole. From thefe properties I thought it was fimply a folution of foap, of which it ftill retained the fmell and tafte, which had been decompofed by an acid; but fubfequent experiments foon convinced me that there was fomething more in it.

Chemical examination.

A turbid part is feparated by repeated filtration.

Firft Experiment.—To find whether I could feparate the white matter which rendered the liquid turbid, by filtration, I put a certain quantity of it on bibulous paper: it paffed turbid and milky for a long time, but, by returning it feveral times on the fame filter, I fucceeded in obtaining it as clear as

* From Bulletin des Sciences, Tom. III. p. 210.

as water; and I afterwards examined the liquor, and the matter which remained on the filter, feparately.

Second Experiment.—If my conjecture was well founded, I fhould only find in this liquor the bafe of a foap united to an acid, of which there was a fuperabundance. My firft care was to fatisfy myfelf of the nature of this acid, and that which its tafte had already indicated to me, was confirmed by muriate of barytes producing in it an abundant precipitate, infoluble in nitric acid: thus I was convinced that the liquor contained fulphuric acid; but, on the other hand, ammonia having produced in this liquor a white, flocculent, femi-tranfparent precipitate, I faw that it contained fomething more than the falt refulting from the decompofition of foap.

Contains fulphuric acid.

Ammonia forms a white precipitate in it.

Third Experiment.—I then precipitated a certain quantity of this liquor; I wafhed the precipitate and dried it: as it had all the phyfical characters of alumine, I combined it with fulphuric acid; I added to it a little fulphate of potafh, and, by a flow evaporation, obtained fome very fine alum. We have therefore already found in this liquor alumine and fulphuric acid, doubtlefs combined in the ftate of alum.

Contains alumine, probably in the ftate of alum;

Fourth Experiment.—It was now requifite to know whether the liquor from which I had feparated the alumine, did not ftill contain fome other fubftance, and I therefore fubmitted it to fome trials by the re-agents, among which the oxigenated muriatic acid and the infufion of nut-galls enabled me to difcover a new body: the firft rendered the liquor turbid, and foon afterwards produced white flakes in it; the fecond produced yellowifh-white flakes, in much greater abundance than thofe arifing from the effect of the muriatic acid; whence I fufpected that, befides the matters mentioned above, this liquor contained an animal matter, and more particularly gelatine.

and alfo an animal matter,

Fifth Experiment.—To be better fatisfied of the nature of this fubftance, I evaporated the liquor to drynefs, by means of a gentle heat; I obtained a yellowifh falt, of a bitter tafte, which, by being re-diffolved in water, left a voluminous yellow matter, in the form of flakes, very glutinous, and acquiring, by drying, a fort of elafticity. This fubftance, placed on burning coals, fwells and exhales white fumes, which have the odour of ammonia and fetid oil, fuch as are generally given by animal matters.

I no

which is gelatine.

I no longer doubted that a certain quantity of animal gelatine had been put into this compofition, with the intention of giving greater vifcofity to the liquor, and enabling it to fupport, for a longer time and more effectively, the parts of the oil in fufpenfion. It is probably by heat and by a commencement of decompofition, that the animal gelatine had become infoluble in water; but I perceived that the liquor containing the falt alfo held fome of it in folution, for the muriatic acid and infufion of galls ftill formed precipitates in it, only lefs abundant than the firft time.

Examination of the fat fubftance;

Sixth Experiment.—In this experiment I endeavoured to learn the nature of the fat fubftance retained in the filter, and of which I have fpoken above: my intention was principally to difcover whether it held fome other fubftance in combination.

For this purpofe I burned it with the filter in a platina crucible; it exhaled a vapour fimilar to that of tallow or the oils; it left fome afhes, of which the filter had furnifhed a part: I found a fmall quantity of alumine in them, which could only proceed from the oil, for the bibulous paper did not contain an atom. I am alfo of opinion, that, in addition to the alumine, this oil contained a fmall quantity of animal matter, but I cannot pofitively affert it.

which retained fome alumine in combination.

Thus, notwithftanding the excefs of acid which exifted in the liquor, the oil in precipitating carried with it, and retained in combination, fome alumine, and probably fome animal gelatine.

It is a combination of oil, alumine, and animal gelatine;

Hence the fubftance which, by uniting with ftuffs, renders them *impermeable* to water, is not the oil alone, but a combination of this fubftance with the alumine, and probably with the animal gelatine, which renders this property more durable.

and yields a little foda and fulphate of potafh.

Seventh Experiment.—The liquor which I had fucceffively deprived of the oil, the alumine, and, in part, of the animal matter, by the different proceffes mentioned above, gave me, by a flow evaporation, cryftals of a falt compofed of foda and fulphate of potafh.

Another analyfis.

Eighth Experiment.—I made a better analyfis of this liquor by another procefs, which I fhall detail very briefly.

I precipitated the alumine and the oil by lime-water; I collected, wafhed, and calcined the precipitate: that which remained in the crucible was alumine and lime.

 The

The liquor, from which these matters had been separated, evaporated to a certain degree, yielded sulphate of lime, a certain quantity of animal matter, become insoluble by the desiccation of the liquor, and, finally, sulphate of soda and potash, containing also animal gelatine, soluble in water.

The following is the manner in which I conceive this liquor was prepared, *with the exception of the proportions:* Soap and glue, or any other gelatine, was dissolved in water; a solution of alum was mixed with the solution of these substances, which, by its decomposition, formed in the mixture a flocculent precipitate, composed of oil, alumine, and animal matter; afterwards some dilute sulphuric acid was added, to redissolve the alumine in part, to render the precipitate lighter, and to prevent it from falling: but the alumine being once combined with the oil and animal matter, will not dissolve again entirely in the sulphuric acid, which is the reason why the oil continues to be very opake, and neither rises nor precipitates: it will be understood, that too much sulphuric acid is not to be added. I do not know if this is precisely the manner in which it is made; I only know, that by following this process, I succeeded in composing a liquor exactly resembling it, and which possessed the same properties. Manner of preparing the liquor.

X.

New Experiments on Absorption by Charcoal, made by Means of a new Machine. *By* C. L. Morozzo.*

1. IN the second memoir which I published on charcoal, in 1783,† I mentioned my intention of employing its property of absorbing the gases, to form a good eudiometer. I immediately set about the construction of an instrument with which I made some experiments; but the agitation of the times obliged me to abandon my pursuits: a philosopher, however, to whom I had shown this instrument, took the liberty of publishing it, notwithstanding its imperfections, and without naming the author: First notice of the invention.

* From the Journal de Physique, Floreal, An. XII.

† Journal de Physique, p. 376.

author *. Let us paſs over theſe unpleaſant events, as well as the alterations and corrections which I made to bring the inſtrument to its preſent ſtate: the deſcription which follows, and the figure will be ſufficient to ſhow what it now is.

Deſcription of the Inſtrument. (*Plate* XIV.)

Deſcription of the inſtrument.

II. A B is a tube of clear glaſs, three lines and a half in diameter and eighteen inches high: to this tube a ſtop-cock C is fixed, to ſhut the communication, by means of the key D, ſo that the tube may be filled with water or mercury, and the gas to be examined may be afterwards paſſed in. To this ſtop-cock C a large one E is ſoldered, an inch and a half in diameter, and almoſt three inches long. The handle F F ſerves to turn the key of the ſtop-cock E; this key has a hole, one inch and three lines in depth, and ſeven lines in diameter, (*Fig.* 2.) in which the charcoal is placed. This key is perfectly tight, and, when turned, communicates with the cock C, *Fig.* 3, not permitting the paſſage of any air whatever. The large ſtop-cock is encloſed in a wooden frame, and well ſecured by means of the nut H, which confines the lower end of the ſtop-cock E behind the board; the lower extremity of the tube dips into a veſſel of mercury I I; the board M M keeps the inſtrument perpendicular; N N are pads which ſerve to raiſe the cup filled with mercury; O O is a ſcale mounted on a ſlip of wood, which is graduated to inches and lines. The endleſs ſcrew P ſerves to raiſe or lower this ſcale, to bring it to the level of the mercury in the cup.

III. After having uſed the inſtrument for ſome time, it is neceſſary to unſcrew the keys to clean them, for, by uſe, duſt, charcoal, or aſhes will get in, which deranges the inſtrument, and ſuffers air to paſs.

IV. From ſeveral experiments, I found that the ſtop-cock D C, which is attached to the cryſtal tube ſhould be of ſteel, for on examining the reſidual air, or filling the tube with mercury to examine any gas, the mercury will at length attack the copper.

* The author does not name the perſon here alluded to; and in a note he gives a deſcription of this firſt imperfect inſtrument, referring to the figure ſo publiſhed. This is omitted, becauſe of little utility and not intelligible without that figure. N.

V. I

V. I alſo think it right to propoſe a ſmall alteration: it is that the extremity of the tube which is plunged into the bowl of mercury, ſhould be a little more bent, that the reſidual gas may be transferred with greater eaſe: this ſlight variation, however, is not at all injurious to the inſtrument, becauſe the heights are meaſured by that of the mercury, the level of which is indicated by the ſcale prepared for that purpoſe; its greateſt inconvenience will be to make a larger and more cylindrical bowl neceſſary. Propoſed improvement.

VI. This machine was made by J. B. Piana, a very ſkilful mechanic; he conſtructed it with ſuch accuracy, that it preſerved the moſt perfect vacuum for eight days, which is what I am unacquainted with in pneumatic machines. Accuracy of the inſtrument.

Manner of uſing the Inſtrument.

VII. Suppoſe I am deſirous of examining the abſorption effected in atmoſpheric air by charcoal. The tube A B is filled with the atmoſperic air of the place in which the machine is: the ſtop-cock D is open, the upper one is cloſed: I place the cup of mercury under the tube, into which I inſert a ſmall glaſs ſyphon, to cauſe the air to take the level of the mercury. If I make uſe of a gas, I pump out the internal gas of the tube with a glaſs ſyringe, to bring the level within the tube to that of the exterior mercury in the bowl. Afterwards, by means of the ſcrew P, I move the ſcale till its zero is alſo at the level of the mercury. Method of uſing it.

I make a piece of charcoal, weighing, half a dram or thirty-ſix grains, red-hot, and with tongs place it in the cavity G of the large ſtop-cock; I then turn the handle F F, and in a few minutes the mercury will be ſeen to aſcend more or leſs in the tube, according as the air is more or leſs pure.

If it is wiſhed to examine other gaſes, or atmoſpheric air brought from another place, then cloſe the key D, and fill the tube A B with water; afterwards diſplace the water with the gas, in the common way by means of a transferrer; bring it to the mercury, and, by means of bibulous paper, abſorb the water perfectly; then wipe the outſide of the tube carefully with warm flannel, and leave the inſtrument at reſt for two hours: or otherwiſe make uſe of a mercurial apparatus, which is preferable.

Inconveniences of the Inſtrument.

Inconveniences.

VIII. Nothing is more certain than the property poſſeſſed by charcoal of abſorbing a more or leſs conſiderable quantity of gas, and of atmoſpheric air; the experiments which I have publiſhed are a complete demonſtration of it; but I then made uſe of white glaſs tubes, hermetically ſealed at the upper extremity, and paſſed the charcoal under the mercury, by which its interſtices were filled with that fluid. At preſent, on endeavouring to make comparative experiments with my new machine, which I was deſirous ſhould be a perfect eudiometer, I found that the cavity G, in which I placed the charcoal, and the ſmall ſpace between the two keys of the ſtop-cock, contained air; that by the heat of the charcoal this air was dilated, and partly expelled, and that conſequently, on opening the key of the ſtop-cock, a ſmall abſorption was occaſioned by the vacuum thus made, and the mercury then roſe. To convince myſelf, and to diſcover at what quantity this abſorption might be calculated, the apparatus being arranged as for other experiments, inſtead of charcoal I introduced a piece of red-hot pumice-ſtone (I gave the preference to this ſubſtance, becauſe I was certain that it had not the property of abſorbing the air, and alſo that it would not take a greater heat than charcoal): on turning the key an abſorption of an inch took place, and from ſeveral experiments, I am convinced that this quantity is uniform within half a line.

Correction of the Inſtrument.

Correction of the inſtrument.

IX. From the experiment made with the pumice-ſtone, it is obvious that if an inch is deducted from the total abſorption, the real abſorption produced by the charcoal will be obtained.

Beſides, in comparative experiments this imperfection cauſes no error; becauſe as they are all conducted in equal circumſtances, the variations produced, either by the different gaſes, or by the different quality of the charcoal made uſe of, will be always proportional.

X. With this inſtrument I made ſeveral experiments which I ſhall now relate.

Order of the experiments.

XI. I began by examining the action of charcoal when cold, and afterwards at a low heat; I then examined the abſorption by

by charcoal expoſed, for a greater or leſs length of time, to the light of the ſun; this enabled me to aſcertain the quantity of light and caloric which enter into the charcoal.

XII. The experiments on atmoſpheric air, which is more or leſs abſorbed according to its purity, follow next.

XIII. After theſe are the experiments on different gaſes.

XIV. Oxigen gas gave me very ſingular reſults, diametrically oppoſite to thoſe I had obtained in tubes hermetically ſealed, and when I paſſed the charcoal through the mercury, which deranged me greatly in the conſequences I deduced from it.

XV. After this article will follow ſome conſiderations on the quality of the charcoals made uſe of, which prove that their property of abſorbing is different according to their different qualities.

XVI. I afterwards ſhow that my inſtrument may be uſed in many caſes, if inſtead of mercury, coloured water is employed, but with requiſite modifications.

XVII. The charcoals which had been employed in the experiments all acquired a greater weight: I endeavoured to extract the air which they had abſorbed.

XVIII. I alſo mention the trials which may be made by ſaturating the charcoal with different ſubſtances, having only made four experiments.

XIX. I then proceed to the concluſion, in which I endeavour to give the explanation of theſe experiments. The opinion which I had adopted in the ſecond memoir, receives additional confirmation from it; but the reſults of the experiments I made on oxigen gas having changed the ideas I had formed, I am not willing to hazard an explanation at preſent. Philoſophers will therefore be contented with having new facts and new experiments.

The experiments which I detail were repeated ſeveral times, and their accuracy may be depended on, as I can aſſure my readers, that I have always conſidered truth as the brighteſt ornament of a philoſopher.

Experiments made with my new Inſtrument.

1ſt. The charcoal which I generally employed was that of beech-wood; the weight of the pieces half a dram, that is to ſay, thirty-ſix grains.

Experiments with the new inſtrument.

2d. The tube of my glafs was eighteen inches long, from the bottom of the key to the furface of the mercury, the level of which is indicated by the moveable fcale. The diameter of the tube is three lines and a half: it contains one ounce and feven drams — five grains of water, or a volume of atmofpheric air of four grains and a half at a temperature of $+10°$.

3d. Being defirous of throwing a light upon the property poffeffed by charcoal of abforbing a part of the atmofpheric air, as well as of feveral gafes, by means of my new inftrument, I made a great number of experiments, which I fhall now defcribe.

4th. Charcoal, when cold, has the property of abforbing fome fmall portions of atmofpheric air, and this abforption, although very flow, is not complete in lefs than twenty-four hours.

On charcoal heated without the contact of fire,

5th. Wifhing to try whether charcoal to which heat had been communicated without direct expofure to fire, would fhow any abforbent power, I placed a piece in a fmall matrafs, which I immerfed in boiling water for an hour; having afterwards put the charcoal into the machine, I obtained an abforption of about three inches.

6th. I boiled oil, in which I left the matrafs for an hour; an experiment with this charcoal gave an abforption of three inches two lines.

7th A fmall matrafs containing a piece of charcoal was immerfed for an hour in boiling alkaline lixivium: it gave an abforption of three inches three lines nearly.

Thefe three experiments fhow that heat communicated to charcoal, even without the contact of fire, gives it the property of abforbing a larger portion of air.

and expofed to the folar light.

8th. The preceding experiments induced me to examine whether the light of the fun would communicate the fame property to charcoal, I therefore expofed different pieces to this light, in a white porcelain bowl, and the abforptions were proportionate to the times which the pieces were expofed to the light of the fun *.

* At the inftant of putting a piece of charcoal into the machine, I placed another which had been expofed with the reft to the fun for three hours, on the bulb of a thermometer; it only raifed one degree and a half of Reaumur's fcale: the heat therefore was not very confiderable.

After

After two hours the charcoal abſorbed one inch two lines.

After three hours, one inch eight lines.

After five hours, two inches.

After ſeven hours, three inches nearly.

It is difficult to obtain very preciſe reſults, becauſe the heat of the ſun is greatly varied, according to the wind; but in this caſe it is light as light which unites with the charcoal, for in very cold days when the thermometer in the ſhade was at — 6°, I ſtill obtained an abſorption *. The light unites with the charcoal.

9th. Being deſirous of aſcertaining whether this abſorbent property belonged to charcoal excluſively, I tried ſeveral other bodies, which there was reaſon to believe contained a great deal of the matter of fire.

10th. I took a ſmall cylinder of loaf ſugar, cold; it did not produce the ſmalleſt apſorption. Experiments on loaf ſugar,

A ſimilar piece, which I had heated in a matraſs with boiling water, gave an abſorption of between two and three lines.

A piece of ſugar which had been expoſed to the ſun for two hours alſo gave an abſorption of between two and three lines.

11th. I then proceeded to the examination of ſulphur and ſealing wax. on ſulphur,

A cylinder of ſulphur of the ſame weight gave no abſorption when cold.

A ſimilar cylinder, which had been expoſed four hours to the light of the ſun, gave an abſorption of about three lines.

A piece of cold ſealing wax did not give the leaſt abſorption, and a ſimilar piece which had been two hours in the ſun, gave an abſorption of about two lines. and on ſealing-wax.

12th. All theſe ſmall abſorptions which were obſerved as well in the ſugar as in the ſulphur and ſealing wax, were only owing to the ſlight heat of theſe bodies, which produces a dilatation in the air of the cavity, and a diſplacing of that portion of the air which is filled by the ſolid body; for a piece of pumice ſtone, of cork, or of any other body, produces the

* It would be intereſting to examine the abſorptions which might be produced by different pieces of charcoal which had been in the light of the ſun tranſmitted through coloured glaſſes, or by making the different coloured rays fall on the charcoal by means of a priſm, and to obſerve whether the piece expoſed to the red ray would abſorb more or leſs than that which had received the violet ray, and ſo of the others.

ſame

Charcoal alone poffeffed of this abforbent property.

fame effect. It is therefore in charcoal alone that this abforbent property refides, and it is developed to the higheft degree by heat and light.

Experiments on Atmofpheric Air.

Experiments on atmofpheric air.

13th. I then proceeded to examine the effects of incandefcent charcoal on atmofpheric air, in my inftrument, and I repeated the experiments feveral times: the air of my garden was conftantly lefs abforbed than that of my bed-room, an hour after the windows had been opened, and this was lefs abforbed than that of my bed-room in the morning before opening the windows: the abforptions were,

The air of the garden, feven inches fix lines.

The air of the room after the window had been open, eight inches one line.

The air of the room, in which it had not been renovated, eight inches fix lines.

Former experiments.

14th. In the experiments which I made in the year 1783, with tubes twelve inches in height, and one inch in diameter, I paffed the charcoal through the mercury; the abforption of the atmofpheric air was then always about three inches and fix lines, that is to fay, a little more than one fourth: in this cafe it is uniformly greater and about one third. Probably the mercury filled many of the interftices of the charcoal when it was paffed through it, which might prevent this greater abforption.

Air of a privy.

15th. I afterwards examined the air of a privy, which was taken into the fyringe feven toifes above the fewer.

The abforption was eight inches, that is to fay, nearly equal to that of my room. This did not furprize me, becaufe in thofe compofitions which contain azote, this gas is not mnch abforbed, as we fhall prefently fee: befides, Mr. White with an eudiometer of nitrous gas alfo found that the air of a privy gave him an abfoption equal to that of common air *.

Experiments on the Gafes.

Experiments on the gafes.

16th. After having examined the atmofpheric air, I proceeded to the examination of carbonic acid gas; I filled the

* See Journal de Phyfique, Tom XVIII. p. 145, and the reflections of Guyton Morveau in his excellent treatife on the means of difinfecting the air.

tube

tube with this gas, which I had extracted from powdered marble by vitriolic acid. I placed the incandescent charcoal in the machine which effected an absorption of sixteen inches six lines, that is to say, of eleven twelfths of the capacity. This experiment corresponds exactly with that which I made with the tube of twelve inches in height, and one inch in diameter, and in which the charcoal was passed through the mercury: the absorption in that case was eleven inches.

Carbonic acid gas.

17th. I afterwards examined the effect produced on the same gas by a piece of charcoal which had remained five hours in the solar light. It caused an absorption of ten inches and three lines, which surprized me greatly.

18th. But to ascertain whether charcoal has any peculiar affinity with carbonic acid gas, I tried this gas by placing a piece of cold charcoal in the machine, and the absorption was nine inches six lines.

19th. To discover the effect of carbonic acid gas when mixed with atmospheric air, I filled the tube, which is eighteen inches high, with nine inches of carbonic acid gas, and nine inches of the air of my room with the windows open: the charcoal effected an absorption of twelve inches and three lines.

A mixture of carbonic acid gas and atmospheric air.

On analysing this experiment, it agrees perfectly with those related above, for

The absorption of the air of my room was	8 inches	1 line.
———————— of the carbonic acid gas	16	6
Total	24	7

The half of this is twelve inches three lines and a half, within half a line of the absorption obtained.

20th. I afterwards tried the action of charcoal in hidrogen gas, obtained from iron by sulphuric acid, and the absoption was three inches and one line: it was accomplished in five or six minutes, and stopped at that height: the effect observed in my machine did not surprize me, for I had noticed in my first memoir, that of all the aeriform fluids, hidrogen gas was absorbed the least. There is a perfect correspondence between this experiment, and that which I made in the year 1783, in crystal tubes of one inch in diameter and twelve in height: the absorption was then two inches and one line, that is to say, one sixth of the height; in my new machine, the tube of which is eighteen inches long, the absorption was three inches and one line, which is also the sixth of the capacity.

Hidrogen gas.

21st. On

21ſt. On trying the ſame hidrogen gas with a piece of charcoal which had been kept about four hours in the bright ſolar light, I had only an abſorption of ſix lines.

Azote gas.

22d. I next proceeded to the examination of azote gas. I began by examining that obtained from atmoſpheric air, in which a candle was extinguiſhed.

The abſorption was ſix inches.

23d. I examined that obtained by the combuſtion of ſulphur, and had an abſorption of ſeven inches and three lines: it is to be obſerved that this gas always contains ſulphureous acid, and for that reaſon more of it was abſorbed.

A ſecond experiment with the charcoal gave me ſeven inches and two lines.

I afterwards tried the azote gas obtained from atmoſpheric air in which I had extinguiſhed a large piece of charcoal under a bell: the abſorption was ſeven inches ſix lines; but this gas always contains a little carbonic acid, and conſequently there is a larger abſorption.

24th. To be ſure of having the pureſt azote gas without mixture, I took the gas obtained by decompoſing nitrous acid by means of hidrogen gas, which laſt gas had been obtained from the decompoſition of water by iron.

The abſorption was only ſix inches and one line.

We have not yet very correct ideas with reſpect to azote: it appeared to me, however, in ſeveral of the experiments which I made with atmoſpheric air, that when it is united with aqueous vapour, more of it is abſorbed than when in a dry ſtate. Beſides it is evident that the azote gas is not much abſorbed, which correſponds with my firſt experiments.

(The Concluſion in our next.)

XI.

On the Commerce of Hens Eggs, and on their Preſervation. By M. Parmentier *.

The bulk of the egg does not depend ſo much on the nourifhment as on the breed of the hen.

THERE are very conſiderable differences in the eggs of hens in reſpect to ſize. Some are as large as the eggs of ducks, while others reſemble thoſe of pigeons. M. Parmentier, from obſervations

* From Bulletin des Sciences, Tom. III. p. 214.

obſervations on a great number of hens of different breeds, which he reared in the ſame place, has found that the bulk of the eggs depends much more on the breed of the hen than on the quantity of nouriſhment. Thoſe breeds which give the largeſt eggs are not however to be preferred on that account, becauſe with them as much may be loſt in the quantity of eggs as is gained by their ſize. Of all the breeds known in France the author gives the preference, with reſpect to the produce of eggs, to that which is called the common hen, and which is only common becauſe its merit is known. Thoſe which have black legs are in greater eſteem than thoſe with yellow ones.

From comparative experiments continued for a year, M. Parmentier found that though the eggs of this breed were not ſo large as ſome others, yet every thing beſides being equal they produced at leaſt one half more.

Next to this breed of hens comes the creſted hen and the large Flemiſh hen. The one is more delicate eating, becauſe laying leſs than the common hen, it grows fatter; the other, without being more productive, is preferable for raiſing chickens. The *ſilky* hen *(poule de ſoie)* ſo beautiful in its form and in the fineneſs of its plumage, ſo careful in laying, ſo aſſiduous in ſetting, ſo tender of its chicks, might be recommended, but unfortunately two of its eggs are not worth one common egg. This circumſtance places it among thoſe which muſt be left for the curious. Superior breeds.

After the choice of the breeds, care muſt be taken that the hens are neither fed too abundantly nor too ſparingly; that they do not wet their feet, that they are ſufficiently cloſe in the rooſt to heat and electrify each other; and, that they meet with a little warm dung in the day time. Management of the hens.

When the only object in keeping hens is to procure eggs, and thus turn to profit the grain which remains among the chaff and in the dung-hills, it is wholly uſeleſs to keep cocks at the ſame time, becauſe experience has ſhown that hens deprived of the male do not lay leſs than thoſe which have it. The ſaving by this is not the only nor the greateſt advantage. The unfecundated eggs keep much better than thoſe which are fecundated. It has been found by experiment that they can ſupport a heat of 32° (87° F.) for thirty or forty days without experiencing any change. It is therefore evident that the evaporation of the liquors is not, as Reaumur thought, the immediate cauſe Unfecundated eggs leaſt liable to ſpoil.

caufe of the putrid change in eggs, and that to preferve them, it is not fufficient to cover them with greafe or oil, as this learned man recommends, fince in the experiment mentioned above the unfecundated eggs did not fpoil, though they loft confiderably by evaporation. The fecundation, by the principle of life which it communicates to the germ, expofes the eggs to many accidents which do not take place with thofe in which the male has had no fhare.

Caufes of the fpoiling of eggs.

M. Parmentier particularizes fome of thefe accidents. Some of them arife from the commencement of the developement of the germ. Sometimes it is enough if feveral hens lay their eggs in the fame neft; for the egg which is firft laid in it, partaking in fucceffion, and for fome hours, of the heat of the hens which follow each other, undergoes a fpecies of incubation which excites the vitality of the germ, and this egg becomes changed, though it is but recently laid. It is thus that eggs of the fame age appear frequently to differ in their frefhnefs. At other times the change in the egg may arife from the fecundating germ being killed, either by thunder, or in the conveyance, by the jolting of the carriage or the rolling of the veffel, or by the lapfe of time. When the germ is once dead it corrupts, and communicates the corruption to that which furrounds it. This theory appears to explain one method I made ufe of fuccefsfully for preferving eggs even when fecundated: it confifts in plunging them for a couple of feconds into boiling water. It is known that by this means they become fufceptible of being preferved for feveral months, if they are afterwards kept in a cool place, or in falt. M. Parmentier fufpects that the utility of this procefs depends on the deftruction of the vitality of the germ by the boiling water.

Utility of immerfing the eggs for a fhort time in boiling water.

Eggs laid at fea keep longer than others.

Mariners affert that eggs laid at fea keep better than others. May not this arife from the hens on board a fhip having no communication with cocks? For the fame reafon the diminifhed vigour of the cocks in our poultry yards, in autumn, may be one caufe of the eggs laid in that feafon being more capable of being kept than thofe of the firft laying, to which may be added, that the hens are at that time fed more with grain and lefs with herbage.

Requifites for keeping eggs frefh.

From thefe obfervations, M. Parmentier thinks that the firft condition towards obtaining eggs capable of being preferved and

and tranfported without fpoiling, is not to keep a cock with the hens in the poultry yard. It is an error to fuppofe that eggs not fecundated have a worfe tafte than thofe which are fo. The author afcertained that the moft delicate palate was incapable of difcovering any difference. Afterwards it is only requifite to fhelter the eggs from humidity, light, heat, and froft. The method which fucceeded beft with the author was to procure bafkets made of ftraw, in which he placed the eggs with layers of chaff between them. The ftraw and the chaff are dry materials, fmooth, very bad conductors of heat, and confequently, very proper to preferve the character of frefhnefs in the eggs: thefe bafkets were afterwards fufpended in a dry, dark and airy fituation.

XII.

Method of giving the Colour, Grain, and Hardnefs of Steel to Copper. By B. G. Sage*.

MARGRAFF and Pelletier have publifhed their inveftigations relative to the union of phofphorus with different metallic fubftances; the French chemift has brought this procefs to perfection: it was by repeating and varying his experiments that I difcovered that the readieft and moft certain method of phofphorating copper is to take the copper in the metallic form, to melt it with two parts of animal glafs, and a twelfth part of powdered charcoal. But it is effential that the copper fhould offer a large furface; an advantage which is obtained by ufing chips of this metal, which are to be placed in alternate layers with the animal glafs mixed with the powdered charcoal. I expofe the crucible to a fire brifk enough to melt the animal glafs; it forms phofphorus; the greateft part of which burns, while another part combines with the copper, in which it is fo confolidated that it does not quit it, although it is kept in fufion for twenty minutes under the animal glafs which has not been decompofed.

The readieft and moft certain method of uniting copper and phofphorus.

When the crucible has cooled and is broken, the phofphorated copper is found under the glafs, which has paffed to the

Phofphorated copper.

* From Journal de Phyfique, Meffidor, An. XII.

4 ftate

ſtate of a red enamel, in the form of a grey and brilliant button: on weighing it, it is found to have acquired one twelfth by this operation.

Its action on poliſhed iron.

If the melted phoſphorated copper falls on a plate of poliſhed iron, it ſpreads itſelf in the form of plates of various configurations which are irideſcent like the neck of a pigeon.

Is more fuſible than copper.

Phoſphorated copper is much more fuſible than red copper: it may be often melted under charcoal powder without loſing its properties.

Does not quit the phoſphorus without difficulty.

The ſame phoſphorated copper, expoſed for a long time under a muffle, does not ſeparate from the phoſphorus without great difficulty.

Reſembles ſteel in hardneſs, grain and colour, and does not tarniſh by expoſure to the air.

The copper thus combined with the phoſphorus acquires the hardneſs of ſteel, of which it has the grain and the colour; like it, it is ſuſceptible of the moſt beautiful poliſh; it is turned eaſily in the lathe; it does not change in the air: I have for fifteen years, kept buttons of poliſhed phoſphorated copper in my laboratory, which have not experienced the leaſt change. The copper does not develope any ſmell on being rubbed: if it was ductile it would be of the greateſt utility, ſince fat bodies do not ſeem to make any impreſſion upon it.

A ſmall quantity of charcoal to be uſed in its preparation.

In the phoſphoration of copper there is only a part of the animal glaſs decompoſed: becauſe a ſufficient quantity of charcoal is not employed to convert all the acid into phoſphorus; but it is neceſſary that this ſhould be the caſe, that the phoſphorated copper may ſeparate and collect with facility.

Red enamel.

The deep red enamel which is formed in this experiment may be advantageouſly employed for porcelain or the enamels, ſince it does not change colour in the fire.

Copper and phoſphorus can only be combined in the dry way.

Copper cannot combine with phoſphorus except in the dry way. If a cylinder of phoſphorus is put into a ſolution of nitrate of copper diluted with four or five thouſand parts of water, at the end of eight days the copper will be found in a metallic form, cryſtallized and ductile, forming a caſe for the cylinder of phoſphorus.

Obſervations

XIII.

Obſervations on Mr. Gough's *two Letters on Mixed Gaſes.**

To Mr. NICHOLSON.

SIR,

MY reply to Mr. Gough's ſtrictures being chargeable, it seems, with acrimony and ridicule, and with having but few arguments, and thoſe in appearance negligently conducted, (though the laſt charge requires a whole letter to make it good); I propoſe, in what follows, to avoid the two former of theſe as much as may be, and to be as careful as poſſible in conducting my arguments; ſo, that if they appear to Mr. G. deſtitute of that logical preciſion which characteriſes his, he may aſcribe it to my inability, and not to any want of inclination. Introduction.

The accuracy of Mr. G.'s demonſtration in the former letter, depends upon that of three phyſical data: *1ſt*, The ſpecific gravity of azotic gas; *2d*, The ſpecific gravity of oxigenous gas; and, *3d*, The quantity of oxigen in a given volume of atmoſpheric air. If any *one* of theſe be wrong, it may prove fatal to his demonſtration: now it unfortunately happens that all *three* are wrong, and that, when corrected, they prove the very reverſe of his propoſition; namely, that *atmoſpherical air is a mechanical mixture of oxigenous and azotic gaſes.* Obſervations on Mr. Gough's letter on mixed gaſſes.

Mr. G.'s data are,

Sp. gravity of azotic gas,	.985
———— oxigenous gas,	1.103
Quant. of oxigen in atmoſphere, *per cent.* in bulk,	22-25-28, uncertain which.

The true, or at leaſt the moſt approximate numbers, are,

Sp. gravity of azotic gas, -	.966
———— oxigenous gas,	1.127
Quant. of oxig. in atmoſphere, *per cent.* in bulk, - -	21

Then, *per* Mr. G.'s theorem, $21 \times 1{,}127 = 23.667$
and $79 \times .966 = 76.314$

$$99.981$$

* See Pages 107, 160, of the preſent Vol.

The

Obfervations on Mr. Gough's letter on mixed gafes.

The fum is fo near 100, that Mr. G. will not venture to prefume any thing on the difference, further than that the data are ftill in a fmall degree incorrect, which I believe no body will difpute with him.

Mr. G. by this time is ready to query, How do you know your data to be more correct than mine? I will now inform him.

Dr. Prieftley was perhaps the firft to inveftigate the fpecific gravities of the two gafes in queftion. His method was very exceptionable; but fuch is generally the cafe in the infancy of any fcience. *(See Vol. II. Page 452, abridg. Ed. of his Nat. Philof.)* He found azotic gas as much lighter than common air as oxigenous was heavier. Mr. Kirwan foon after gave a much nearer approximation; namely, the one which Mr. G. has adopted. Lavoifier alfo found the fpecific gravity of feveral gafes. *(See Elements of Chemiftry, Append. Table 7).* His refults nearly agree with Kirwan's in regard to oxigen, but differ confiderably in regard to azote. Laftly, Mr. Davy, when inveftigating the compounds of azote and oxigen, found it expedient to afcertain with precifion the fpecific gravity of the two gafes. Having every means of his predeceffors, and their refults before him, he ought at leaft to have decided in regard to the difference between them. Accordingly he finds his refults to agree with Lavoifier's in refpect to azote; but he finds the fpecific gravity of oxigen fomewhat greater than either of them. *(See Refearches, pag. 565).*

The following table exhibits the refults of all thefe together, reduced to the ftandard of atmofpheric air.

Specific Gravities of

	Azotic Gas.		Atmof. Air.		Oxigenous Gas.
According to Prieftley,	.989	—	1.000	—	1.011
Kirwan,	.985	—	1.000	—	1.103
Lavoifier,	.966	—	1.000	—	1.102
Davy,	.966	—	1.000	—	1.127

As for Mr. G.'s third datum, I think it fcarcely pardonable in the prefent day, for any one to pretend to difcufs a queftion concerning the conftitution of the atmofphere, under the uncertainty whether it contains 22 or 28 *per cent.* of oxigen. He ought to be acquainted with the hiftory of eudiometry,

and

and to repeat experiments confidered as decifive, efpecially as they are of a fimple nature. All the chemifts of Europe feem to be agreed, that either 21 or 22 *per cent.* in bulk, is the proper number. My own experience gives 21 for the neareft integer, which agrees with Mr. Davy's in your Journal, (quarto Series), Vol. V. page 175: if Mr. G. entertains any doubt on the fubject, I would refer him to that paper, and he will receive ample fatisfaction.

Obfervations on Mr. Gough's letter on mixed gafes.

After what has been faid, I think Mr. G. muft be fatisfied that he has virtually demonftrated the atmofphere to be a mechanical mixture of the two gafes: If, however, he ftill alledge that I only oppofe one authority to another, and that his is as good as mine; then I would recommend him to fatisfy himfelf as follows: mix 21 parts (or a quantity of gas containing 21) of pure oxigen, and 79 of pure azote, together; after this proceed to the analyfis of the gafes, and examine the differences between the refults, and thofe done from the analyfis of 100 parts of atmofpheric air.

I cannot conclude this article without obferving, that Mr. G. muft have been totally unacquainted with the opinions of chemical philofophers on the fubject, or he would not have expanded into the compafs of five or fix pages a fimple argument, which has often been adverted to by others, and is now wholly abandoned as untenable. Mr. Davy, in his Refearches, publifhed in 1800, advocating the notion of atmofpherical air being a chemical compound, produces four evidences, one of which is ftated as follows: *See Page* 326.

" *2dly*, The difference between the fp. gr. of atmofpheric air, and a mixture of 27 parts oxigen and 73 nitrogen, as found by calculation; a difference apparently owing to expanfion in confequence of combination." In a note he adds, " The two firft evidences have been often noticed." This gentleman however foon after, finding that the atmofphere contained only 21 *per cent.* of oxigen, muft have feen that this evidence was not to be admitted. Since that time it has not been urged by any one to my knowledge.

I come next to Mr. G.'s reply at page 160. This at the commencement purports to be a defence of the charge, that *my arguments are but few, and negligently conducted*; at the conclufion it is afferted to be an *anfwer to all my objections, and fomething more.* This language may be that of logical precifion

Obfervations on Mr. Gough's letter on mixed gafes.

precifion with Mr. G.; but it would have been more intelligible to me if he had ftated the objects of his reply thus:

1ft, A refutation of Mr. D.'s arguments in favour of his fyftem.

2d, An anfwer to his objections brought againft mine.

3d, New objections to his fyftem.

The firft thing worthy of notice is the objection to my argument for the mutual penetrability of gafes. I have affumed *one* poftulate and taken *two*, and the latter of them is erroneous, namely, that *all gafes are porous*. It is true, I have taken two poftulates into the argument, without exprefsly requiring both; the former being peculiar to my theory, was neceffarily demanded in a formal way; the latter being the refult of all experience, and never in any one inftance having been found to fail, I thought it might *tacitly* be affumed. However, the judgment of philofophers muft be fufpended on this head, as Mr. G. it feems, is about to prove that *no gas is porous*, and that *a cubic foot of one gas cannot be put into a veffel that is previoufly occupied by another gas.* Mr. G. furely cannot be ferious in this objection; but merely ufes it to gain time, and means to turn it off with a laugh, that he has at leaft produced one *folid* argument againft my *airy* hypothefis.

Mr. G. finds it extremely convenient for his purpofe, that I fhould grant him the following poftulate: "If a particle of vapour can pafs freely through the air, a fecond can alfo fucceed it at any given diftance." I certainly cannot concede fo indefinite a demand; but it will perhaps be of equal ufe to him to have the following: If d be the diftance of two particles of vapour of the temperature of 212°, and preffure 30 inches; then, at the temperature of 60° or upwards, if one particle of vapour can pafs freely through the air, a fecond may fucceed it at any diftance greater than $4\,d$.

As for the important argument which I confider equivalent to a demonftration of the nature of vapour, and of its relation to gafes, and entirely inimical to the notion of chemical affinity, Mr. G. has not ventured to revive it: Probably he has fomething in referve on this head. I mean the argument derived from the fact, that a vacuum, or any kind of air of any denfity whatever, takes up juft the fame quantity of any vapour.

The

The propofition refpecting the mechanical action of air on the furface of water, will be anfwered by Mr. G. when he has proved that *air has no pores*, or no *capacity for the reception of water*. For, nothing can be more clear than that, whatever may be the preffure of the atmofphere, and however few the points of its action, water cannot be forced into the pores of air, if it have none.

Obfervations on Mr. Gough's letter on mixed gafes.

Mr. G. proceeds to ftate two new facts, which are faid to be inexplicable on my principles: I muft undertake the arduous tafk. The facts are, *1ft*, Air containing aqueous vapour is fpecifically lighter than air without it, *cæteris paribus*; and, *2d*, A bottle internally moift, containing air, being heated, more air is expelled than if the bottle had been dry. Both granted.—Now for the explication. The fpecific gravity of aqueous vapour has been found by De Sauffure, Watt, and others, to be about $\frac{2}{3}$ or $\frac{3}{4}$ of that of atmofpheric air in like circumftances; by fome experiments of my own I am induced to think it is nearly .7, that of air being 1. Let the temperature be 64°, and the air be filled with vapour as much as poffible in the temperature, in which cafe $\frac{1}{50}$ of the elaftic force will be due to the vapour. *(See Manch. Mem. Vol. V. P. 2, page* 559); then, by the theorem fo elaborately exemplified in Mr. G.'s former letters, we have $\frac{49 \times 1 + 1 \times .7}{50} = \frac{49.7}{50} = .994$, for the fpecific gravity of common air filled with vapour at 64°, when that of dry air of the fame temperature would be 1. Thus it appears that my hypothefis not only explains the fact of diminifhed fpecific gravity, but accounts for the quantity of diminution. Can Mr. G.'s theory of chemical folution do this?

In the fecond cafe we find heat generating vapour, which increafing in quantity and force with the temperature, diffufes itfelf through the air in the bottle; the hand being occafionally removed from the mouth, fuffered the extra-portion of air and vapour to ftart out; juft as if there had been a generation of a like portion of oxigen, or any other permanent gas, inftead of the vapour; in which cafe a portion of both does certainly efcape. Mr. G. finds the vapour generated this way between 59 and 126°, to be rather lefs than $\frac{1}{9}$ of at-

mofpheric

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mofpheric force; it ought to be juft $\frac{1}{5}$ by the table above referred to. But it is obvious, that by removing the hand occafionally, the *full* quantity of vapour for any temperature could never be obtained in this way, no more than pure oxigen could be procured in the bottle by a fimilar procefs. The grand queftion with Mr. G. however, is, How does vapour of half an inch force, expand the pores of air fubject to 30 inches of preffure? And his anfwer feems to be, it is impoffible, according to the axioms of dynamics. This queftion is what I fhall now confider.

Having myfelf ftudied the principles of dynamics, as well as thofe of many other mathematical and phyfical fciences, under the tuition of Mr. Gough, I feel under ftrong obligation to him; but thefe, he will readily grant, do not bind me to fubfcribe to his opinions, when I cannot perceive them to be well founded. He charges me, in the prefent inftance, with a miftake in regard to dynamics; but he has not pointed out any particular axiom which I have offended: The miftake, I think, is with him, and fhall endeavour, in what follows, to point it out.

It is a principle in dynamics, that whenever a fyftem of bodies act upon each other, and are in a ftate of equilibrium, the *leaft* force impreffed upon any one difturbs the equilibrium. Thus, the ocean and the air, though bound to the earth by its fuperior attraction, are neverthelefs difturbed by the feebler influence of the moon. Air in a bottle is a fyftem of particles at equal diftances repelling each other, but in equilibrium by the gravity of the incumbent atmofphere; confequently the *leaft* force impreffed upon them muft difturb that equilibrium. Now, *ex hypothefi*, air does not repel vapour at a diftance, but only in contact; therefore vapour can be formed in fuch a fyftem: when once formed, it meets with no elaftic refiftance or repulfion but from particles of its own kind; therefore it is conftantly tending outwards, where the particles of its own kind are leaft denfe: in its way it infringes upon particles of air, and exerts fuch force as it is capable of upon them: a number of particles of air are thus gently propelled in the direction of the ftream, and the reft of the fyftem are obliged to accommodate themfelves in order to preferve the equilibrium: thus the diftances of the particles of air are gra-

 dually

dually increafed, and that in proportion to the force which the vapour exerts. Therefore vapour of the leaft poffible force can, in fuch circumftances, extend the pores of air. Q. E. D.

I remain your's,

J. DALTON.

Manchefter, Nov. 15, 1804.

XIV.

Some Account of a Condenfer of Forces, or a Method of obtaining the greateft poffible Effect from a firft Mover, of which the Energy is fubject to Increafe or Diminution within certain Limits; and in general to vary at Pleafure the Refiftance to which the Effort of the firft Mover forms an Equilibrium in any Machine whatever, without changing any Part of the Conftruction. By R. PRONY.*

THE problem of mechanics, of which the folution is here given, is one of the fmall number of thofe which, leading to refults independent of the particular mechanifm of the machine to which they are applied, prefent, in their folution, a generality which may be compared with that of the rational mechanics, or analyfis. Problems in mechanics,

It may be enunciated in the following terms:

" Any machine being conftructed, to find, without making any change in the conftruction, a means of tranfmitting to it the action of the firft mover, by fulfilling the following conditions; *viz.* enunciated.

" 1. That it may be poffible at pleafure, and with great fpeed and facility, to vary the refiftance (againft which the effort of the firft mover muft continually make an equilibrium) in limits of any required extent. Conditions;

" 2. That the refiftance being once regulated, fhall be rigoroufly conftant until the moment when it is thought proper to increafe or diminifh the fame.

" 3. That in the moft fudden variations of which the effort of the firft mover may be capable, the variation in velocity of the machine fhall never undergo a folution of continuity."

* From the Bulletin of the Philomathic Society at Paris, No. 83.

Solution exhibited in an engine.

I shall here apply the solution which I have discovered of this problem, to the dynamic effect of wind; it will be easy to make the same general when the other first movers are used.

Problem in dynamics: To oppose a constant weight to a variable first mover, and transmit the force, &c.

The section and plan of the machine are exhibited in Plates XV. and XVI. O O represents the vertical arbor to which windmill sails are adapted; *e e e e* is an assemblage of carpentry, of which one of the radii O *e*, bears a curved piece *b d*, of iron or steel: Vertical axes of rotation *a a a*, being placed round the axis O O, also divide the circumference in which they are found into equal parts.

Each of these axes carries a curve *a f*, of iron, steel, or copper; so situated, that when the wind acts upon the sails, the curve *b d* presses against one of the curves *a f*, and causes the vertical axis to which this last curve is fixed, to make a portion of a revolution.

The curves *b d* and *a f* must be so disposed, that when *b d* ceases to press on one of the curves *a f*, it shall at the same instant begin to act upon the following curve: the number of axes which are provided with these curves, must be determined by the particular circumstances of each case; and it is also practicable to substitute, instead of *b d*, a portion of a toothed wheel having its centre in the axis O O, and to place portions of pinions instead of the curves *a f*, but the dispositions represented in the figure are preferable.

Each of the axes *a a a a* (which are all fitted up alike, though, for the sake of clearness, only one of them has its apparatus represented in the drawing), each of these carries a drum or pulley *t t r r*, on which is wound a cord that passes over a pulley *p*, and serves to support a weight Q by means of the lever F G, upon which this weight may be slided and fastened at different distances from the point of motion G.

The same axes *a a* pass through the pinions *q q*, to which they are not fixed; but these pinions carry clicks or ratchetts, which bear against the teeth *r r*; so that, when the weight Q tends to rise, the ratchett gives way, and no other effect is produced on the pinion *q q*, either by the motion of the axis or of the drum *t t r r*, excepting that which causes the ascent of the weight *q q*. But the instant that the curve or tooth *b d* ceases to bear against one of the curves *a f*, after having caused the corresponding weight Q to rise, that weight Q tends to redescend, and then the toothed wheel *r r* acts against the

Problem in dynamics: To compose a constant weight or a variable first mover, and transmit the force, &c.

the ratchett, ſo that Q cannot deſcend without turning the pinion $q\,q$ along with the drum $t\,t\,r\,r$.

The pinion $q\,q$ takes in the wheel $a\,b$, from the motion of which the uſeful effect of the machine immediately reſults; ſo that the effect of the deſcent of one of the weights Q, is to ſolicit the wheel A B to motion, or to continue the motion in concurrence with all the other weights Q, which deſcend at the ſame time. This wheel A B carries beneath it oblique or bevelled teeth G D, which take in a like wheel C E, and cauſe the buckets at S to riſe.

The alternation in the motion of theſe buckets may be effected by the mechaniſm I have deſcribed in the firſt volume of the Memoirs of the Inſtitute.

From the preceding deſcription it is ſeen that the machine, being ſuppoſed to ſtart from a ſtate of repoſe, the wind will at firſt raiſe a number of weights Q, ſufficient to put the machine into motion, and will continue to raiſe new weights while thoſe before raiſed are fallen; ſo that the motion once impreſſed will be continued.

Among the numerous advantages of this new mechaniſm we may remark the following:

1. No violent ſhock can take place in any part of the mechaniſm.

2. The uſeful effect being proportioned to the number of weights Q, which deſcend at the ſame time, this effect will increaſe in proportion as the wind becomes ſtronger, and cauſes the ſails to turn with more velocity.

3. The weights Q being moveable along the levers F G, it will always be very eaſy to place them in ſuch a manner as to obtain that ratio of the effort of the firſt mover to the reſiſtance, as ſhall produce the maximum of effect.

4. From this property it reſults, that advantage may be taken of the weakeſt breezes of wind, and to obtain a certain product in circumſtances under which all other windmills are in a ſtate of abſolute inactivity: this advantage is of great importance, particularly with regard to agriculture: the windmills employed for watering lands are ſometimes inactive for ſeveral days, and this inconvenience is more particularly felt in times of drought. A machine capable of moving with the ſlighteſt breeze, muſt therefore offer the moſt valuable advantages.

I ſhall

I ſhall give a more ample account of this apparatus in a memoir which I ſhall preſent to the Inſtitute, as ſoon as the machine which I am now erecting in the country ſhall be completed.*

XV.

Abſtract of a Memoir on the Poſſibility of obtaining Pruſſiate of Potaſh free from Iron; the Unalterability of the Pruſſic Acid at high Temperatures; and the true Nature of the Combinations of this Acid with different Baſes.† *By* Bucholz.

Utility and advantages of pruſſic compounds.

THE combination of pruſſic acid with ſalefiable baſes, forms a claſs of ſalts of the greateſt utility to the analytical chemiſt; for by means of them he is not only enabled to aſcertain whether a metallic ſubſtance be preſent in any ſolution which forms the ſubject of his reſearch, but alſo what metal is preſent, as well as its reſpective quantity. But in order to be accurate in this reſpect, the re-agent employed muſt itſelf be free from metallic admixture; or the quantity and nature of the metal it contains muſt at leaſt be known. To accompliſh this, chemiſts have hitherto laboured in vain. To remedy theſe defects, Mr. Bucholz has inſtituted a number of experiments which led to facts hitherto unknown; and as they are highly important, we ſhall exhibit the reſults of the principal ones, which are as follows:

* The practical mechanic may perhaps find it an advantage to be informed, that the whole effort of a firſt mover cannot be tranſmitted by this, or any other method of raiſing weights, in order to operate by their fall. If the wind had been employed to raiſe a maximum of weight through a given ſpace in a given time, this weight would be leſs than would continue in equilibrio at reſt againſt the ſame force, as is well known; and if this weight be again employed in like manner to raiſe another maximum of weight, this alſo will be leſs than the former, &c. For this reaſon it is that the fly has been uſed as an equalizer of ſteam-engines, in preference to the older method of raiſing water and ſuffering it to deſcend on an overſhot wheel. W. N.

† Abridged from a memoir in Gehlen's new Journal of Chemiſtry, Vol. I. Part IV. Page 406, by — A.

1. Pruſſic

1. Pruſſic acid can only be formed during the carbonization of blood or animal matter, *at a red heat*: We, therefore, need not be afraid of heating the mixture of blood and alkali, in the preparation of pruſſiated alkalies, to incandeſcence.

Pruſſic acid is formed only at red heat.

2. The affinity of pruſſic acid with alkalies is greater at high temperatures, and in the *dry way*, than at low temperatures, and in the *humid way*.

Its affinity with alkalies is then greateſt.

This obſervation diſtinguiſhes pruſſic acid from the reſt of the ſo called animal and vegetable acids, and their combinations with alkaline baſes; for all of them are deſtroyed at a red heat. Hence it is obvious,

All other vegetable acids are deſtroyed by ignition.

3. That in the preparation of pruſſiated alkalies, the preſence of water ſhould be avoided as much as poſſible.

Water ſhould be avoided.

4. The direct combination of pruſſic acid with alkalies, cannot be accompliſhed.

P. acid and alk. do not directly combine.

5. Pure pruſſiated alkalies are decompoſable by the affuſion of water; part of the pruſſic acid eſcapes, and may eaſily be recogniſed by the odour of bitter almonds, and frequently alſo by that of ammonia.

P. pruſſiated alk. decompoſable by water.

6. The combination of pruſſic acid with alkalies can only be effected *at all temperatures* up to a red-heat, by the interpoſition of a portion of oxide of iron; and the affinities conſiſting between the pruſſic acid and alkali, are retained with a greater force in the humid way, in the ratio of the ponderable quantity of iron preſent.

P. acid does not combine with alk. at low temp. but by medium of iron, &c.

7. All the precipitates obtained in chemical analyſis by means of pruſſiated alkalies prepared in the uſual manner, contain more or leſs iron, without exception; whereas the ſame precipitates, produced by the action of abſolutely pure pruſſiates, are free from that metal, and of a different colour than the former.

Common pruſſiates contaminate metals with iron; pure p. give a different reſult.

8. The affinity of oxide of iron to charcoal, is more powerful than the joint attraction of pruſſic acid and potaſh to that metal; hence we always find that the greateſt quantity of that metal remains behind with the charcoal, in the uſual proceſs for obtaining pruſſiate of potaſh.

Moſt of the iron preſent remains in the coal, when p. alk. is made.

9. The precipitability of the oxides of metals by pruſſiated alkalies, is in the ratio of their oxidability, or quantity of oxigen they contain.

Metals are precip. by p. alk. in the order of oxigenation.

Such are the obſervations of this chemiſt. Other leſs intereſting facts will be omitted in the preſent abſtract.

Experimental

Experimental Enquiries concerning the beſt Method of preparing Pruſſiate of Potaſh free from Iron.

Preparation of pruſſiate of alkali, pure. *Low ignition* of dried blood and potaſh, and ſolution by water.

Four oz. dried blood were intimately mixt with a ſolution of potaſh (containing one ounce of potaſh), then evaporated to dryneſs, ignited to redneſs, till no more flame undulated at the ſurface of the ignited maſs. The ignited maſs was diffuſed through ſix ounces of water, and the ſolution filtered. The filtered fluid was colourleſs, it contained an exceſs of alkali, and emitted a ſtrong odour of bitter almonds. It yielded, on being evaporated as expeditiouſly as poſſible, a ſaline maſs, conſiſting of a mixture of pruſſiate and ſub-carbonate of potaſh. In order to ſeparate theſe two ſalts, one drachm of the ſaline maſs was introduced into a vial containing a mixture of half an ounce of highly concentrated alcohol and half a drachm of acetic acid, of 1,056 ſpec. grav. On agitating the fluid no ſenſible effervefcence took place, but much pruſſic acid was diſengaged; a proof the pruſſic acid was retained with a leſs affinity by the alkali than the carbonic acid. On examining the reſidue, which had been treated with acetic acid and alcohol, it was found to contain only a ſmall portion of pruſſiated alkali.

The ſalt was treated with acetic acid and alcohol.

Very little pruſſiate, and not ſeparable from the carbonate by thoſe agents.

From this experiment we learn, that one part of potaſh cannot be converted into pruſſiate of potaſh, by being heated to redneſs with four of blood; and that the quantity of either free or carbonated alkali, under theſe circumſtances, cannot be ſeparated from the pruſſiated potaſh by means of acetic acid and alcohol.

Leſs of blood was then uſed in the ignition.

In order to learn if a leſs quantity of blood would not be more advantageous for the production of pure pruſſiate of potaſh, four ounces of dried blood, and two of carbonate of potaſh, were heated to redneſs in a crucible till no more flame appeared. On covering the mixture with charcoal powder, and again heating it, a prodigious evolution of ammonia took place; a phenomenon I do not venture to explain. The maſs, after having been diffuſed through water, filtered and evaporated, yielded a crop of cryſtals, conſiſting of pruſſiate and ſub-carbonate of potaſh, the former predominating conſiderably.

Great evolution of ammonia.

The ſubcarbonate and pruſſiate not ſeparable by alcohol and acetic acid.

On ſubjecting this mixture of ſalts to the joint action of acetic acid and alcohol, it was found impoſſible to ſeparate the pruſſiate of potaſh from the ſub-carbonate.

Being

Being thus perſuaded that a leſs quantity of blood was not more advantageous for forming pure pruſſiate of potaſh, I again mingled two ounces of dried blood with one of carbonate of potaſh: this mixture was treated as before, with the exception that the maſs, after the flame had entirely diſappeared, was ſtrongly ignited for three quarters of an hour. The obtained maſs, after refrigeration, weighed nine drachms. Digeſted cold with four ounces of water, and filtered and evaporated, it afforded *a dark coloured* fluid. On dropping into a conſiderable quantity of muriatic acid, no blue, but a white precipitate fell down, which was inſoluble in muriatic acid: Acetic acid occaſioned no change in this dark-coloured fluid; and on mingling it with ſulphuric acid, and evaporating the mixture to dryneſs, and re-diſſolving the maſs in water, it yielded a ſmall quantity of pruſſian blue; a proof that iron was preſent in this fluid.

Blood and alkali, as in the firſt experiment, *ſtrongly ignited* for three quarters of an hour.

The fluid of ſolution was dark, and contained ſome iron, though very little.

The quantity of iron being very ſmall, but the colour of the fluid very dark when compared with thoſe of the former proceſſes, it was natural to ſuppoſe, that the colouring-matter could not be attributed to the minute quantity of iron preſent, but that perhaps a portion of charcoal was diſſolved in the fluid: to inveſtigate which the following experiments were inſtituted:

A like quantity and like proportions of blood and carbonate of potaſh, as ſtated laſt, were gradually heated to incandeſcence, and the fire gradually augmented, *until the maſs began to fuſe* on the ſides of the crucible. The maſs, after having been diffuſed through water and filtered, yielded a much darker-coloured fluid, which, when mingled with muriatic acid, yielded a pearl-coloured precipitate. After being mixt with muriatic or ſulphuric acid, evaporated and rediſſolved in water, it afforded a conſiderable quantity of oxide of iron; a proof that the union of the pruſſic acid with potaſh is permanent at very high temperatures, but that this combination, under ſuch circumſtances, exerciſes a ſtrong action upon the oxide of iron contained in the blood.

Repetition of the experiment with fuſion of the maſs.

The pruſſic acid remains united with alk. at very high temp. and takes iron ſtrongly.

Theſe facts were proved by repeating this experiment, but taking care to expoſe the maſs to a ſomewhat leſs degree of heat. The fluid now obtained was leſs coloured, and yielded leſs oxide of iron.

Leſs oxide taken up by leſs heat.

This

Experiments with pure alkali.

This being proved, the author varied his experiments, so as to be thoroughly convinced of the facts. He also endeavoured to ascertain, whether alkalies, freed from carbonic acid, were better calculated for the production of prussiated alkali; and, if possible, to find out the proper proportions of ingredients for obtaining this salt. With that view, a quantity of solution of potash, containing one ounce of alkali freed from carbonic acid, was mixt with three ounces of blood, and evaporated to dryness. (It was of a colophony colour, soluble in water, and emitted, on being heated, a very strong odour of ammonia). On being transferred into a crucible, and gradually heated till no more flame appeared, and diffused through four ounces of water, it yielded a *limpid* fluid, of a strong alkaline taste, and odour of bitter almonds. The usual experiments proved, that it contained comparatively little prussiated alkali, but a large quantity of carbonate of potash.

Low heat produced little combination of p. acid and the alkali.

Exp. to determine the proportions of carbonate and blood requisite to produce the prussiate, &c.

Being thus convinced that this experiment proved fruitless, it was deemed necessary to ascertain the respective quantity of oxide of iron contained in the prussiated alkali, that might be produced from a given quantity of blood and carbonate of potash.

The experiments with low heat.
4 oz. blood and 1 alk. carb.

With that view, four ounces of dried blood were mixt with a solution of carbonate of potash, containing one ounce of carbonated alkali: the mixture, after being evaporated to dryness, was heated till the flame ceased to appear. It now weighed $1\frac{1}{2}$ oz. On being elixiviated with eight ounces of water, it afforded a fluid of a very pale yellowish brown, or wine colour: Its taste was alkaline, mixt with that of bitter almonds. Two drachms of water mixt with 20 drops of a concentrated solution of muriate of iron, when decomposed by this prussiated alkali, afforded four grains of prussian blue, stript from the adhering oxide of iron by muriatic acid.

Product very little.

2 oz. blood, 1 carb. alk. low heat. Product not increased.

Two ounces of dried blood and one of carbonate of potash, treated in a similar manner, yielded, by being mingled with a like quantity of muriate of iron, $4\frac{1}{2}$ grains of pure prussian blue.

(The Remainder in our next.)

Observations

XVI.

Observations on the Cause which augments the Intensity of the Sound in Speaking Trumpets. By J. H. HASSENFRATZ *.

ALTHOUGH the speaking-trumpet is an instrument which has been long known, since Kircher is of opinion that Alexander used it to command his army, and Solard made one in Paris, in 1654, from the description which Kircher had given of that of Alexander, it was not, however, until 1671, when Morland made known that which he had constructed, and invited men of science to determine the figure most proper for this instrument, that the speaking-trumpet was really known among us, and began to be used.

Antiquity of the speaking trumpet.

Morland's.

It appears that Morland did not adopt the conical form terminated by a mouth piece, which he gave to the speaking trumpets of glass, iron and copper, he made until after a succession of trials on the best form to be given to them, to make them produce articulate sounds.

It also appears that it was without knowing it, and by chance, that Cassegrain gave the speaking trumpet, which he made in 1672, the hyperboloidal form first noticed by Sturm.

Cassegrain's.

Until 1719, when Hasse published a dissertation on the improvement of speaking trumpets, this instrument was constructed without principles; for we neither can nor ought to consider as principle, the harmonic proportion mentioned by Cassegrain as being necessary between the length and the width of the tubes of speaking trumpets.

Were at first constructed without known principles.

The law of the reflection of sound in echoes, led Hasse to apply the theory of catoptrics to speaking trumpets, and induced him to consider the combination of the *ellipsoidal* and *paraboloidal* forms as the most advantageous for this instrument: but this union not producing the effect which the professor of Wirtemberg had hoped for, it was abandoned, and in conformity with his opinion, the augmentation in the intensity of the sound continued to be considered as the product of the reflection of the sonorous rays in the speaking trumpet, and of the vibration of the substance of which it was composed. It was for this reason that, in the fabrication of these instruments, the

Hasse's theory of the augmentation of the intensity of sound in them.

* From Annales de Chimie, Praireal, An. XII.

attention

attention was directed to having a hard, elaſtic and thin ſubſtance, that it might vibrate in uniſon with all the tones, and reflect the ſonorous rays which ſtruck it in all directions.

Lambert's theory. Vibration injures the articulation of the ſounds.

In 1763, Lambert withdrew from the theory of ſpeaking trumpets, the vibration of the materials of which they are compoſed: he ſhowed that the vibration of the ſides which is calculated to augment the intenſity of a long continued ſound, would render articulate ſounds confuſed, which muſt ſucceed each other rapidly; that in this caſe therefore it would be neceſſary to ſpeak extremely ſlow, and that, even by ſpeaking ſlowly, it would be impoſſible to diſtinguiſh the conſonants, which are only momentary modifications of the vowels; that the latter, pronounced in the trumpet, would be ſo ſonorous, that the conſonants muſt be gueſſed at, which would be extremely difficult. By ſprinkling the exterior ſurface of a tin ſpeaking-trumpet with ſaw-duſt, I ſatisfied myſelf that the ſurface vibrated in ſome circumſtances, but I was equally ſatisfied, on covering the outſide of the trumpet with a ſoft looſe ſtuff to ſtop and obſtruct its vibrations, that the intenſity of the ſound was no leſs ſtrong, in this ſecond caſe, and, that thus the vibration of the ſubſtance of the trumpet was, at leaſt, uſeleſs, if it was not injurious to the diſtinctneſs of articulated ſounds.

Speaking trumpets do vibrate, but the ſound is equally ſtrong when the vibration is prevented.

In his memoir, publiſhed among thoſe of the Academy of Berlin, for the year 1763, Lambert has attributed all the augmentation of the intenſity which the ſound experiences by ſpeaking into a trumpet, to the reflection of the ſonorous rays on the ſmooth and poliſhed ſurface of the interior of this inſtrument; he ſays that, of all the forms, that beſt calculated to concentrate the ſound by reflecting it, and the moſt eaſy to conſtruct, is the conic form: he afterwards ſhows that the ſound is ſtrengthened in conical ſpeaking trumpets, in the proportion of double the length of the cone which forms the trumpet to double the ſize of half the angle at the ſummit of the cone, ſuppoſing the length of the cone to be equal to the radius. If the angle of the cone is made $= \phi$, the ſound is ſtrengthened in the proportion $\sqrt{2} : 2\ (\text{ſin.}\ \tfrac{1}{2}\phi)$.

Proportions of a conical ſpeaking trumpet.

Seeking, from theſe formulæ, to diſcover what would be the beſt proportions to give to conical ſpeaking trumpets, he found that whatever the angle of the ſummit of the cone was, its baſe muſt be equal to the diſtance between the ſummit of the

the cone and the mouth of it, and that its length muſt be equal to the diameter of the mouth, divided by four times the ſquare of the ſine of half the angle of the cone: that thus in a trumpet of ſix feet in length, the angle of the cone muſt be 16° 17, and that ſuch a ſpeaking-trumpet would make the ſound 96 times more intenſe.

Lambert therefore, as well as thoſe who have preceded him, conſidered the augmentation of the intenſity of the ſound in ſpeaking trumpets, as the reſult of the action of the reflection of the ſonorous rays againſt the interior ſides of the inſtrument.

Theſe inſtruments have latterly been conſtructed without attending to the cauſes of the increaſe of ſound.

It would appear that ſince Lambert's memoir, ſpeaking trumpets have been made uſe of, without attending to the cauſes which augment the intenſity of the ſound, and, in fact, it is ſeen that in all works in which this inſtrument is ſpoken of, its effects are attributed either to reflection alone, or to reflection combined with the vibration of the ſubſtance of which it is formed.

Theory of ſpeaking trumpets and other inſtruments.

If we compare the theory of ſpeaking trumpets with that of the inſtruments which have ſome reſemblance to it, ſuch as trumpets and hunting horns, we muſt be aſtoniſhed to find that they are referred to different principles. In the ſpeaking trumpet, the cauſe is attributed to the reflection of the ſound; in the other inſtruments to the vibration of the air contained in the tube. Why theſe two cauſes, while the effects are analogous? This analogy has led me to inquire if, in reality, the reflection produced the augmentation of the intenſity of articulation in horns and acouſtic tubes, as well as in ſpeaking-trumpets, as is generally believed.

Does reflection increaſe the ſtrength of ſound?

Animals do not hear by the reflection of ſound.

On examining the ears of animals, it is ſeen that the greateſt number have an exterior auricle, which moſt of them direct towards the place from which the ſound proceeds; this auricle being, in many animals, covered inwardly with hair, which ſtops and hinders the reflection of the ſound, it is reaſonable to conclude that it is not by reflection that the ſound is tranſmitted into the ear.

Ear-trumpets do not tranſmit ſound by reflection.

When afterwards we obſerve the form given to acouſtic horns, which is that of a cone, the ſummit of which, ſlightly truncated, is placed in the ear, we are quickly led to conclude that the ſound is not tranſmitted by reflection; for the angle of the incident ray being increaſed at each reflection by that of the cone, after a number of determinate reflections, the angle

would

would become larger than a right angle, and the ray would return upon itſelf. Thus the greateſt number of the rays would iſſue out of the mouth, after being ſeveral times reflected, and the quantity of the rays which thus reach the ear, and which are proportionalby leſs numerous as the cone is longer, cannot ſenſibly augment the intenſity of the ſound. Nevertheleſs, acouſtic horns do ſtrengthen the ſound conſiderably; this augmentation muſt therefore depend on another cauſe.

Horns of a paraboloid form diſuſed.

It has been propoſed to make horns of a paraboloidal form, as being beſt calculated to tranſmit ſound by reflection; but whether they did not produce the effect which was expected from them, or that they were too difficult to conſtruct, theſe horns have been abandoned, and none but conic horns are made uſe of.

Cylindrical acouſtic tubes tranſmit ſound without reflection to a great diſtance.

The acouſtic tubes employed to tranſmit the ſound to great diſtances, ſo that it cannot be heard by perſons placed between the two extreme points, are uſually formed of cylinders, which ſerve to conduct the ſound. Lambert had already ſtated that reflection did not increaſe the intenſity of ſound in cylinders, becauſe whatever might be the direction of the incident rays, being conſtantly reflected between two parallels, the angle which they form with the axis of the cylinder muſt be the ſame at entering and quitting it; that thus the ſound muſt experience a diminution at its iſſue, which will be ſo much the greater as the number of reflections is more conſiderable. Nevertheleſs the ſound is tranſmitted to a great diſtance by means of theſe tubes, and I have aſcertained that the beating of my watch, which ceaſes to be audible at the diſtance of 1^m. 10, the medium of ſeven experiments *(A)*, is heard at the diſtance of 2^m 25, the mean diſtance of ſeven experiments *(A)* when I place it in the mouth of a paſteboard tube, 0^m. 038 in diameter and 0^m. 6 in length; whence it follows that this tranſmiſſion muſt proceed from another cauſe.

According to the theory of reflection the enlarged part (pavillion) is of no uſe.

All theſe conſiderations have therefore induced me to examine the effects of the ſpeaking trumpet with the greateſt care. It follows from the theory of the reflection of ſound, applied by Lambert to the ſpeaking trumpet, that the opening by which it is uſually terminated, ſhould be at leaſt uſeleſs, if not injurious, for it has no influence on the concentration of the reflected rays; and therefore this gentleman propoſed to lay it aſide entirely. The analogy of form between the trumpet

pet and the ſpeaking trumpet not being a ſufficient reaſon for retaining it, it is probable that it would have been laid aſide, in the ſame manner as the contour of the trumpet was abandoned, when the form and dimenſions moſt ſuitable to this inſtrument were ſought for by experimental trials. There would alſo have been another inducement to diſpenſe with it, becauſe Haſſe had already omitted it in the ſpeaking trumpet which he indicated as preferable to thoſe of Morland and Caſſegrain. I aſcertained by experiment that two ſpeaking trumpets of the ſame length and the ſame diameter produced different effects when they were terminated by an enlarged aperture or not, and that in general this enlargement conſiderably increaſed the intenſity of the ſound. The beating of my watch which I heard at $4^m. 2$, the mean diſtance of ſeven experiments *(a)* by placing it at the mouth of a ſpeaking trumpet of $0^m. 6$ long and $0^m. 38$, mean diameter of the tube, was not heard beyond $2^m. 25$, mean diſtance of ſeven experiments *(a)* when I placed it in a tube of the ſame length and the ſame diameter. Thus the cauſe which occaſions this aperture to ſtrengthen the ſooner is different from the reflection of the ſonorous rays.

Which is contrary to experiment.

Since the reflection of ſound does not in any manner concentrate the ſonorous rays in a cylindrical tube, it muſt follow, as Lambert has concluded in his Theory of Reflection, that ſpeaking trumpets with cylindrical tubes ſhould not ſenſibly augment the ſound. To prove whether experiment agreed with this theory, I immediately conſtructed a ſpeaking trumpet, with a cylindrical tube of $0^m. 36$, the length of the cylinder, $0^m. 25$, the length of the enlarged part, $0^m. 035$, the diameter of the cylinder, and $0^m. 190$, the greateſt diameter of the enlarged part, and, by comparing the intenſity of the ſound which it produced, with that of a conical ſpeaking trumpet of the ſame height, and of a ſimilar mean diameter, I aſcertained that the ſtrength of the ſound was ſenſibly the ſame. In both, the beating of my watch, which I did not hear in the open air beyond $1^m. 08$, mean diſtance of five experiments *(b)* was heard at $3^m. 94$, mean diſtance of five experiments *(b)* when I placed it at the mouth of either ſpeaking trumpet. Since the cylindrical ſpeaking trumpet ſtrengthens the ſound in the ſame manner as that which is conical, and ſince, on the contrary, the theory of reflection ſhows that it ſhould not be ſtrengthened, it follows that the augmentation of ſound in theſe inſtruments ariſes from a cauſe different from reflection.

This is further proved by the ſound being augmented in ſpeaking trumpets with cylindrical tubes, as ſtrongly as in thoſe of a conical form.

One

The intenſity of the ſound is not leſſened by deſtroying the reflection by means of an internal woollen covering.

One deciſive experiment remained to be made with theſe inſtruments, this was to deſtroy and to reſtore the action of the reflection in a ſpeaking trumpet, ſuppoſing it to take place, and to compare the intenſity of the ſound obtained in theſe two circumſtances, in order to determine the proportion of the effect produced by this reflection. I conſequently procured a ſpeaking trumpet of woollen ſtuff, which I placed within one of tin, and I obſerved that the intenſity of the ſound obtained with or without the ſtuff in the inſide, was ſenſibly the ſame; whence it follows that the reflection has no appreciable influence on the augmentation of ſound in a ſpeaking trumpet.

Except inaſmuch as it diminiſhes the diameter of the inſtrument.

I have ſaid that the intenſity of the ſound was ſenſibly the ſame; nevertheleſs I muſt obſerve that it was diminiſhed; for the beating of my watch, placed in the mouth of the bare ſpeaking trumpet was heard at a diſtance of 3^m. 94, mean of five experiments *(b)*, while when placed in the mouth of the ſame inſtrument covered in the inſide with the ſtuff, it was not heard beyond 2^m. 48. This difference, which at the moment might be ſuppoſed to ariſe from the reflection of the ſound taking place in the one caſe, and not in the other, ſeems to depend ſolely on the diminution of the diameter of the cylinder of the ſpeaking trumpet and of the enlarged parts occaſioned by the ſtuff placed within them: I ſatisfied myſelf that the ſpeaking-trumpet which I made uſe of *(b)*, preſerving all its dimenſions and experiencing no difference except in the diameter of the tube, produced a diminution in the intenſity of the ſound, whenever a ſmaller diameter was made uſe of; thus, in the ſpeaking trumpet already mentioned *(b)*, the beating of my watch was heard at a diſtance of 3^m. 94, when the diameter of the tube was 0^m. 035; it was not heard beyond 3^m. 08, when the tube was only 0^m. 032; finally, it was not heard beyond 2^m. 34, when the tube was 0^m. 028. It is evident, therefore from theſe experiments, that the diminution in the intenſity of the ſound by covering the interior of a ſpeaking trumpet with ſtuffs, ariſes from the diminution of the diameter of its tube.

The diminution in the intenſity of the ſound is compenſated by its clearneſs.

I ſhould obſerve that if the internal ſtuff occaſions an inconſiderable diminution in the intenſity of the ſound, it, on the other hand, produces a great advantage by rendering the articulated ſounds clearer and leſs confuſed.

General concluſions.

It follows from all which has been ſhewn on the auricles of animals, on horns, and acouſtic tubes, and on ſpeaking trumpets,

pets, that the augmentation of found, in every circumftance, is not owing to the reflection of fonorous rays, confequently there exifts no reafon for feparating the caufe which augments the found in thefe inftruments, from that which ftrengthens it in trumpets and hunting horns; that the different founds produced in the two cafes are owing to the vibration of the air in the tubes, and their ftrengths or their intenfities, to the augmentation of the amplitude of their vibration, arifing from the greater impulfe which the air neceffarily receives when it is enclofed in a tube. General conclufions.

Experiment (A).

The tubes and the fpeaking trumpet of pafteboard.

The interior diameter of the tube of the fpeaking trumpet is $0^m.038$, that of the exterior diameter of the enlarged part $= 0^m.210$. The length of the tube $= 0^m.4$, and the length of the enlarged part $= 0^m.2$.

	With a tube of the length of			The fpeaking trumpet.	Without tube or fpeaking trumpet.	Obfervations.
	m. 0.4	m. 0.6	m. 0.10			
Diftance at which the beating of the watch was heard.	1.33	1.66	2.33	4.00	1.00	The great difference between thefe experiments arifes, 1ft, from the difpofition of the organ;—2d, from the greater or lefs noife produced in the neighbourhood of the place where the experiments were made.
	2.24	2.90	3.33	4.66	1.16	
	2.16	2.66	3.33	5.00	1.33	
	1.33	1.79	1.85	3.00	0.83	
	1.92	1.95	2.00	5.00	1.33	
	1.58	2.16	2.66	3.66	1.00	
	2.54	2.66	3.17	4.00	1.00	
	12.86	15.78	18.67	29.33	7.65	
Mean	1.83	2.25	2.67	4.20	1.10	

Experiment (B).

The fpeaking trumpet was of tin, as well as the tubes.

The interior diameter of the tube of the fpeaking trumpet $= 0^m.035$, that of the exterior diameter of the enlarged part $= 0^m.290$, the length of the tube $= 0^m.36$, and that of the enlarged part $= 0.25$.

 With

	With an internal diameter of			Without speaking trumpet or tube.
	m. 0.035	m. 0.032	m. 0.028	
				m.
Distance at which the beating of the watch was heard.	4.88	3.58	2.76	1.48
	3.58	2.93	2.28	0.97
	3.25	2.76	2.28	1.30
	4.39	3.58	2.44	0.82
	3.58	2.58	1.95	0.82
Sum	19.78	15.43	11.71	5.39
Mean	3.94	3.08	2.34	1.08

XVII.

Account of Cerium, a new Metal found in a mineral Subſtance from Baſtnas, in Sweden. By W. D'HESINGER *and* J. B. BERGELIUS *.

§ I.

Deſcription of the Tungſtein of Baſtnas.

Diſcovery of a new ſubſtance in tungſtein.

ALTHOUGH this ſubſtance has been formerly aſſayed by Scheele and D'Elhuyar, under the name of wolfram, its conſiderable weight, neverthelefs, determined us to ſubmit it to farther enquiries. Our principal object was to find yttria in it, which, being unknown at the time in which theſe chemiſts operated, might have eſcaped their attention. Our ſuppoſition was not well founded, ſince, inſtead of an earth, we diſcovered a ſubſtance which, according to every appearance, is hitherto unknown, as will be ſeen in the ſequel.

Where found.

The tungſtein of Baſtnas, which we call *cerite,* for reaſons which will be preſently given, was found, in the year 1750, in a copper-mine called Baſtnas, or Saint-Gorans Koppargrufva, at Riddare-Hyltan, in Weſtmannia, of which, with aſbeſtos, it formed the matrix: but after this time it was imbedded in quartz and mica, to the depth of ſeventeen toiſes.

* From the Swediſh, by G. A. Linborn; but here tranſlated from the Annales de Chimie, L. 145.

The

The tungſtein is almoſt always mechanically mixed with black amphibole *(hornblende)*, ſtriated actinote, of a clear green colour *(ſhorl)*, mica, ſulphurated copper, biſmuth, and ſulphurated molybdena, which may be eaſily known by expoſing it to the fire.

Deſcription of the ore.

The cerite, properly ſo called, is tranſparent, of a fleſh colour, ſometimes deep and ſometimes bright, ſeldom yellowiſh. In a maſs, or in ſmall ſpecimens, the ſtone is of an irregular form; its fracture is indeterminate, compact, and a little brilliant, with obtuſe edges; its conſiſtence is tenacious and ſtrong; it gives fire with ſteel with difficulty, but does not ſcratch glaſs; it is not attracted by the magnet; after having been made red-hot in the fire, it loſes its hardneſs, and ſix or ſeven *per cent.* of its weight: by this operation it becomes friable, and acquires a bright yellow colour: it does not melt alone.

Phyſical properties of cerite.

On account of its weight, Cronſtedt has placed it among the tungſteins, in his Mineralogy. In pure fragments, it is to that of water as 4.733 and 4.935 to 1.000. Scheele not having found wolfram in it, called it falſe tungſtein.

Is the falſe tungſtein of Scheele.

The conſtituent principles of this mineral were given by Bergman, in the Memoirs of the Academy for the year 1784, page 121, from an analyſis of D'Elhuyar: they are as follows:—

Conſtituent principles.

Silex, - - -	0.22
Iron, - - -	0.24
Lime, - - -	0.54
	1.00

Heated with borax by the blow-pipe, it forms a globule of glaſs, which, while hot, appears greeniſh, but is colourleſs when cold. Urged with carbonate of ſoda in a platina ſpoon it is not diſſolved.

Habitude with borax.

§ II.

Analyſis of the Proportions of Cerium.

TO ſeparate the yttria which was ſuppoſed to be in it, it was reduced to a fine powder in a porphyry mortar; pure concentrated nitric acid was then poured upon it. The acid was decompoſed, and a conſiderable quantity of nitrous gas

Treatment of the ore with nitric acid, &c.

 and

and carbonic acid gas were disengaged. The stony powder was repeatedly treated with this acid, until the insoluble residue appeared white.

The solution diluted with water was of a yellow colour, which became greenish by boiling, and afterwards red: completely dried, it became of a yellowish white, but regained its red colour by attracting humidity. It is entirely dissolved in alcohol; and the solution, slightly digested, deposits a considerable quantity of oxide of iron. It likewise deposits more oxide of iron by remaining for some days undisturbed in a window. The decanted solution, being almost clear, was evaporated to siccity, and the calcined salt was in the form of a powder, of the colour of bricks. Water could only dissolve the calcareous earth. Distilled vinegar could only take up a very small portion, and was not saturated, though assisted by the heat of ebullition. The evaporated acetic solution gave small granulated crystals, of a saccharine astringent taste. They were not totally soluble in alcohol. The part of the acetous salt which was not dissolved in alcohol, gave, by calcination, a brick-coloured powder, resembling that which had not been dissolved.

Ammonia precipitated the alcoholic solution in a white powder, which became yellowish in the air. It was a little soluble by carbonate of ammonia, and acquired the colour of bricks by calcination. The sediment being separated, the carbonate of ammonia produced a white precipitate, which was pure carbonate of lime. The acetous salt did not therefore contain yttria. The powder from which the calcareous earth had been separated, dissolved in muriatic acid, with a disengagement of oxigenated muriatic acid gas, which indicated that there was a metallic oxide.

Indications of a metallic substance.

Was it oxide of manganese combined with oxide of iron?

To ascertain this, we endeavoured to develope the pure oxide of manganese by means of tartrite of potash, according to Richter's method. We decomposed in this manner, a solution of this substance in muriatic acid, perfectly neutralized by tartrite of potash; and after having washed the precipitate well, we submitted it to a slow calcination; but it only produced the brick-coloured powder.

Does not contain alumine.

Caustic alkali had no action on the insoluble part of the nitrate; which proves that it did not contain alumine.

To

To obtain the pure metallic matter in a ſufficient quantity to make ſeveral aſſays, another portion of cerite was diſſolved in nitric acid, and the ſolution evaporated to ſiccity. Water was poured on the reſidue, and it was precipitated with ammonia. The waſhed precipitate was diſſolved in nitric acid. The ſolution, well neutralized with the alkali, was afterwards precipitated by tartrite of potaſh. A white powder was alſo precipitated from the ſame ſolution by carbonate of potaſh, but it was in ſmall quantity. Theſe precipitates were ſeparately calcined, and both of them acquired the colour of bricks. The precipitate formed by the carbonate of potaſh, was not diſſolved by potaſh aided by digeſtion; it therefore did not contain alumine. The iron contained in the ſolution, precipitated with tartrite of potaſh, was ſeparated by hidro-ſulphuret of ammonia. The remainder of the ſolution of cerite in nitric acid, which had been precipitated by cauſtic ammonia, gave carbonate of lime by carbonate of ammonia.

Component parts of the ore, 50 per cent. of oxide of cerium.

From theſe aſſays it reſults, that cerite contains nearly 23 parts of ſilex, 5.5 of carbonate of lime, 22 of oxide of iron, and a quantity of this metallic matter, the weight of which, after calcination, rather exceeded 50 *per cent.* But this ſubſtance being then, as well as the iron, united with more oxigen than they contained in the cerite, we have, inſtead of a loſs, an augmentation of weight, which probably ariſes from the oxigen. Neither is the loſs which the cerite experienced in the calcination, included in this account. We alſo found traces of manganeſe, but in ſo ſmall a quantity, that potaſh, melted with the cerite and diſſolved in water, did not give any colour.

Not having the practice which complete proportionate analyſes require, we offer theſe reſults with diffidence, and in the hope that ſcientific men of more experience will employ themſelves on this ſubject.

§ III.

Examination of the Metallic Oxide found in the Cerite.

Examination of the oxide.

A PASTE was made of 37 grains of this oxide and linſeed oil, which was reduced in charcoal in a covered crucible. Although it retained ſome carbon, it loſt half a grain of its weight. This maſs was incloſed in a lined crucible without

out any flux, and M. Hjelm expofed it for half an hour to the degree of fire neceffary for the reduction of manganefe. The oxide was not melted, but reduced into a very fine powder: it exhibited brilliant particles in the day-light, and ftained white paper black. It diffolved in muriatic acid, difengaging, at the firft, fulphurated hidrogen gas, and afterwards pure hidrogen gas. This colourlefs folution had a faccharine tafte. Thus it appeared to us that the metal was reduced in part. The origin of the fulphur may be traced to the fulphuric acid, from which the matter had been feparated by the cauftic ammonia. The influence which this acid exercifes in thefe affays, will be feen by the fubfequent enquiries.

M. Gahn, at Fahlun, having more convenient furnaces, has promifed to undertake the reduction of this fubftance with more power; if this operation fucceed, we fhall give an account of it hereafter.

It is the oxide of a metal not yet known.

Thefe appearances, and thofe which follow, determined us to confider the fubftance found in the cerite, as the oxide of a metal hitherto unknown, to which we have given the name of Cerium, from the planet Ceres, difcovered by Piazzi.

Manner of obtaining the pure Oxide of Cerium.

Proceffes for obtaining the pure oxide.

(A.) Pure uncalcined cerite was diffolved in nitro-muriatic, and, after faturating the clear folution with the alkali, was precipitated by tartrite of potafh. The precipitate well wafhed, calcined, and digefted in vinegar, contains the pure oxide of cerium.

Or otherwife decompofe a folution of cerium in nitro-muriatic acid, ftill warm, but not faturated, by fuccinate of ammonia: a fuccinate of iron is gradually depofited. The precipitation is to be continued by means of fuccinate of ammonia, as long as a red precipitate is formed; the folution is then nearly deprived of iron. After having feparated the fuccinate of iron, more fuccinate of ammonia is poured into it, until a white precipitate appears. The folution is then left at reft, in order that the fmall portion of fuccinate of cerium may be depofited. The iron diffolved by the free muriatic acid, is depofited at the fame time, and the folution is freed from this metal. The cerium may afterwards be precipitated by ammonia, and then wafhed and calcined.

Of the Properties of Oxide of Cerium.

(B.) This oxide may appear in different degrees of oxidation. The alkalies precipitate a white oxide from its ſolutions, which ſhows of a yellowiſh colour in the air, but, when perfectly dried, becomes dark. Expoſed to a briſk and long-continued fire, it takes a deep brick-colour. The oxalate and acetate of cerium, calcined in veſſels not completely cloſed, yield a white oxide, which, in an open fire, becomes of the colour of bricks. It does not melt by itſelf. The oxide is capable of different degrees of oxidation.

Treated with borax by the blow-pipe, it melts readily and ſwells. The globule heated by the exterior flame, aſſumes the colour of blood; which, by cooling, paſſes to a yellowiſh green, and at length becomes colourleſs, and perfectly tranſparent. Melted by the interior flame, theſe changes do not take place; it is then reduced into a colourleſs glaſs; but, expoſed for a ſhort time in the exterior flame, the ſame phenomena are produced. If too much oxide of cerium is made uſe of, the glaſs reſembles an opake yellowiſh enamel. Theſe changes are more eaſily manifeſted with the phoſphate of ſoda and ammonia. If two clear and colourleſs globules are melted together, one of which was prepared with borax and the other with the phoſphate, they form a tranſparent glaſs, which, on cooling, becomes opake and pearl coloured. Fuſion with fluxes.

Theſe characters, taken together, ſufficiently diſtinguiſh the oxide of cerium from the oxide of iron. The latter alſo offers the ſame changes of colour, but its glaſs, on cooling, has a deep green colour, *which fades.* The globules made with borax and the phoſphate melted together, yield an opake glaſs, the colour of which is a little deeper.

Oxide of Cerium treated with Sulphuric Acid.

(C.) When oxide of cerium is digeſted with ſulphuric acid, theſe two ſubſtances unite, and the reſult is a red inſoluble ſalt, which is ſulphate of cerium at a *maximum of oxidation.* If the acid is concentrated, it ſcarcely diſſolves any of it. If it is diluted with half its quantity of water, or a little more, the reſult is a yellowiſh oily liquor, which does not adhere to the glaſs, nor does it wet it. If the acid is mixed with ſix or ſeven times its quantity, or even more, of water, and employed in a ſufficient quantity, the oxide is diſſolved of an orange Sulphate of cerium at a *maximum of oxidation.*

orange colour. By a flight evaporation of this folution, it yields fmall, prifmatic, coherent cryftals, of the colour of gold. This falt is an acidulous fulphate of cerium at a *maximum*. Thefe cryftals, thoroughly dried between blotting-paper, and expofed to the air, are gradually reduced to a yellow, almoft cryftalline powder. Rediffolved in water, they experienced a decompofition; a white powder is depofited, and the folution becomes colourlefs. This white powder is a fulphate of cerium, but little oxigenated. If the folution is evaporated to drynefs, it gives an acidulous fulphate of cerium ftill lefs oxigenated. Thefe cryftals are feldom cubical, but almoft always prifmatic, ftriated and collected in bundles. Their tafte is four, but they afterwards become faccharine and aftringent.

Acidulous fulphate of cerium.

Neutral acid.

Acidulous fulphate of cerium but flightly oxigenated.

Muriatic acid deprives the acidulous fulphate of part of its oxigen: As does an increafe of temperature, which afterwards drives off the acid, and forms a neutral fulphate. Calcination reftores the oxigen.

Treated with muriatic acid, the yellow acidulous fulphate of cerium yields part of its oxigen to the acid, which is volatilized in oxigenated muriatic acid gas. The falt remains colourlefs. An augmentation of temperature alone is fufficient for the yellow acidulous fulphate of cerium to lofe its colour by lofing the excefs of its oxigen. If the heat is increafed ftill more, the furplus of the acid is carried off, and a faturated fulphate of cerium remains. By a continued calcination, it regains oxigen, becomes red, and yields a fulphate of cerium at a *maximum*. The fulphate of cerium, difoxigenated by the muriatic acid, is more difficult to re-oxidate by calcination.

Action of the alkalies on the fulphates of cerium.

In the humid way, the alkalies only decompofe the fulphate of cerium incompletely. Ammonia precipitates an oxide from the acidulous fulphate of cerium, which is only in a fmall quantity, but neverthelefs contains part of the fulphuric acid. The fulphate of cerium is not perfectly decompofed, except by calcination with three times its weight of carbonate of foda or potafh. The calcined oxide is of a brown colour. By digeftion, ammonia can deprive it of part of its acid: the oxide takes a diftinct flefh colour, which becomes brighter by drying. Digefted with concentrated muriatic acid, or with nitric acid, a fmall quantity diffolves, having its yellow colour.

Sulphate of cerium and potafh.

If a folution of acidulous fulphate of cerium is precipitated by potafh, a triple combination of cerium, fulphuric acid, and potafh, is feparated, before the acid is faturated. If too much potafh is added, the combination is partly deftroyed. The fulphate of cerium and potafh, at a *maximum*, is of an orange

colour;

colour; that which is at a *minimum*, is white. A ſimilar combination is alſo obtained by pouring muriate of cerium into a ſolution of ſulphate of potaſh. Sulphate of ammonia does not form any precipitate in it; but, on adding a calcareous ſalt to it, ſulphate of cerium combined with potaſh is rapidly depoſited.

Sulphuric acid in exceſs frees ſulphate of cerium from iron.

Theſe characters offer a ready method of ſeparating the ſulphate of cerium from iron. It muſt however be obſerved, that when the ſolutions are ſaturated, a little iron is alſo depoſited, which gives a yellow colour to the precipitate; but by adding a little ſulphuric acid in exceſs, the iron is rediſſolved, and leaves the precipitate entirely white. This combination is only diſſolved in part by dilute ſulphuric acid, and the greateſt quantity of that ſeparates afterwards.

Sulphate, and carbonate of cerium.

The ſulphate of cerium and potaſh melts by a ſtrong heat. Heated with charcoal, it gives ſulphuret of potaſh and ſulphuret of cerium. Melted with carbonate of potaſh, in cloſed veſſels, it yields carbonate of cerium and ſulphate of potaſh. This ſalt contains only one-third of oxide of cerium.

Concentrated nitric acid decompoſes ſulphate of cerium and potaſh.

It is diſſolved in concentrated nitric acid, and, during the cooling, an acid ſalt, formed of acidulous ſulphate of potaſh and a little ſulphate of cerium, cryſtallizes. Thus the ſulphate of cerium combined with potaſh, is decompoſed by concentrated nitric acid. This acid carries off the metal, and the ſulphuric acid is directed wholly upon the potaſh, with which it forms a ſalt with exceſs of acid.

Oxide of Cerium with Nitric Acid.

Nitrate of cerium.

(D.) Nitric acid diſſolves the calcined oxide with difficulty, but that which is precipitated by pure or carbonated alkalies, with eaſe. When the ſolution is ſaturated with oxigen, it is of a greeniſh yellow colour; but colourleſs, when leſs oxided. Evaporated to the conſiſtence of honey, it depoſits lamellated cryſtals, which attract the humidity of the air. The ſolution has a ſaccharine taſte: like all the other ſaturated ſolutions of cerium, it lets fall an oxide of cerium, at a *maximum* of oxidation, in the open air. This precipitate is frequently formed of oxide of iron. When dry, this ſalt is of a yellowiſh white colour; but becomes colourleſs on being diſſolved in a ſufficient quantity of water. It diſſolves readily in alcohol.

A concentrated solution of this salt takes a blood colour, on account of a small quantity of iron, which, by drying, passes to yellowish white, but is restored by a new solution.

A colourless and less oxided nitrate of cerium, is obtained by dissolving the yellowish salt in alcohol: the solution inflames and yields a white salt.

It is destroyed by fire, which drives off its acid.

With Muriatic Acid.

Muriate of cerium.

(E.) The calcined oxide of cerium is slowly dissolved in muriatic acid in the cold, and more readily by heat; an effervescence is produced, owing to a disengagement of oxigenated muriatic acid gas. The taste of the solution is saccharine and astringent; the colour is a very faint greenish yellow: the dried saline mass is yellowish white, and attracts humidity. We only succeeded once in obtaining it crystallized. The crystals were white, brilliant, in four sided prisms, with the points cut off. The salt dissolves readily in alcohol, and its concentrated solution burns with a yellow and sparkling flame. The residue of the salt is white and gives a colourless solution. It is muriate of cerium at a *minimum* of oxidation.

Heated in closed vessels, the water of crystallization is first dissipated, afterwards the acid passes in the form of oxigenated muriatic acid gas. If the operation is stopped before the acid is entirely volatilized, an undecomposed muriate of cerium, at a *minimum* of oxidation remains.

If the muriate of cerium contains iron, it all sublimes in a brown deliquescent mass. Nothing remains in the matrass but a white oxide of cerium, which attracts the humidity of the air, and becomes yellow. Thus, sublimation with muriate of ammonia may be employed to purify a muriate of cerium which contains a little iron.

With Phosphoric Acid.

Phosphate of cerium.

(F.) Free phosphoric acid, saturated with an alkali, precipitates muriate of cerium. The precipitate is white, and soluble in muriatic acid and in nitric acid employed in sufficient quantity.

This salt is also obtained by digesting pure oxide of cerium, moistened with phosphoric acid. It is not soluble in an excess of this acid.

With

With Carbonic Acid.

(*G.*) The carbonate of ammonia precipitates muriate of cerium without effervefcence. After the precipitation, carbonic acid is flowly difengaged in the form of bubbles. The refidue retains its acid, even after deficcation.

Carbonate of cerium.

Dry carbonate of cerium has a white colour tending a little to bluifh or greenifh. It diffolves in the acids with effervefcence. It does not lofe its acid in an open fire. In clofed veffels, without the contact of oxigen, it fupports a gentle calcination, without being decompofed.

With Arfenic Acid.

(*H.*) Free arfenic acid does not produce any change on muriate of cerium. The oxide digefted with this acid, forms an infoluble falt. An excefs of this acid diffolves it, and gives an acidulous arfeniate of cerium. The faturated arfeniate of cerium is depofited in the form of a powder during the evaporation. The refidue does not cryftallize, but by deficcation, becomes a gelatinous, clear, and colourlefs mafs.

Arfeniate and acidulous arfeniate of cerium.

With Molibdic Acid.

(*I.*) The acidulous falts of cerium are not decompofed by molybdate of ammoniac. The molybdate of cerium is precipitated from its faturated folutions, in the form of a white falt, which is not foluble in the acids.

Molybdate of cerium.

With Oxalic Acid.

(*K.*) Either the acidulous or faturated folutions of cerium are precipitated by oxalic acid. According to the degree of oxidation of the metal, the precipitate becomes red or white. This combination is alfo obtained by digefting the oxide with free oxalic acid. An excefs of acid does not diffolve it, but ammonia readily effects its folution, giving it a yellow colour.

Oxalate of cerium.

A fmall quantity of oxide is depofited by evaporation. The folution afterwards yields regular cryftals in the form of needles. Pure alkalies do not occafion any precipitate.

With

With Tartareous Acid.

Tartrite of cerium.

(L.) Free tartareous acid has no action on muriate of cerium. The recently precipitated oxide unites with this acid by digestion, and yields a tartrite of cerium, which dissolves readily in water. This salt is also precipitated from saturated solutions by tartrite of potash. Like the oxilate of cerium, it dissolves in pure ammonia, but does not crystallize.

Tartrite of cerium is not entirely soluble in water; the solution is precipitated by carbonate of soda.

With Benzoic Acid.

Benzoate of cerium.

(M.) This acid does not act on the muriate of cerium; but, by digestion, well concentrated benzoic acid dissolves the oxide of cerium recently precipitated. On cooling, the solution first deposits crystals of the acid in excess, and afterwards benzoate of cerium in the form of a white powder, which adheres to the crystals of benzoic acid.

The resinous matter with which this acid is frequently united, combines with the benzoate of cerium, and forms an insoluble brown powder.

With Citric Acid.

Citrate and acidulous citrate of cerium.

(N.) Muriate of cerium is not precipitated by citric acid. But the oxide digested with citric acid forms an insoluble saturated combination, which an excess of acid dissolves. The acidulous citrate of cerium is of a yellow colour and does not crystallize. Alcohol deprives it of its water, and of part of its acid, but does not dissolve it.

With Acetic Acid.

Acetate of cerium.

(O.) The calcined oxide of cerium is only very imperfectly dissolved in acetic acid, even with the assistance of heat; but that which is recently precipitated by the alkalies, is dissolved with facility. The saturated acetate of cerium is soluble in water; it has a sweet taste, and gives granulated crystals, by evaporation, which do not change in the air, and are but slightly soluble in alcohol.

This salt swells in the fire, and is destroyed.

(To be continued.)

SCIENTIFIC

SCIENTIFIC NEWS.

Extract of a Letter from Mr. BODE, *Astronomer Royal at Berlin, to Mr.* A. F. THOELDEN. *Dated the 23d of October,* 1804.

I HEREWITH transmit to you some of my own observations of the new discovered third planet, (which is to be called *Juno)* made at our Observatory. New planet Juno.

	Mean Time.				Apparent R A.				Declin.			
Sept. 21	-	11^h	55′	21″	-	359°	27′	46″	-	3°	38′	23″ South.
25	-	11	37	4	-	358	47	16	-	4	32	10
27	-	11	28	0	-	358	27	21	-	4	58	35
28	-	11	23	29	-	358	17	18	-	5	11	35
Oct. 10	-	10	28	42	-	356	29	13	-	7	36	56
14	-	10	11	9	-	356	1	14	-	8	18	51
20	-	9	45	22	-	355	29	24	-	9	12	46

I also inform you, that an anonymous admirer of astronomy of Berlin, has deposited with me the sum of 20 Frederics d'or, for any important discovery, treatise, or satisfactory solution of any difficult problem, of, or relating to, astronomy, to be sent to me between this and the end of August, 1805. (The concurrents are requested to write their dissertations either in French, or in German, affixing a motto to each, with a sealed note containing their name and address). After the expiration of the above-mentioned period I am to consult with my astronomical friends respecting the merits of the papers sent, and to adjudge the premium accordingly, on condition, however, that I may be at liberty to make use of such papers, addressed to me, as I think proper to insert into my astronomical journals. Astronomical prize.

Signed, J. E. BODE.

N. B. Any astronomical gentleman who intends to become a competitor, may send his paper to me, and I will forward it to Mr. Bode without any expence.

A. F. THOELDEN.

No. 10, *St. Alban's Street, Pall-Mall,*
Nov. 3*d,* 1804.

Experiments

Experiment on the Heat which is developed during the Compression of the Air *.

Heat by compreffion of air.

A VERY curious experiment has been lately repeated before the National Inftitute. If the air be very rapidly compreffed in the ball of an air gun, a confiderable quantity of heat is difengaged from the firft ftroke of the pifton, which is fo great that it is capable of fetting fire to a piece of fungus match (amadou) placed within the pump. If the body of the pump be terminated by a moveable end formed of a piece of fteel firmly fcrewed in, and furnifhed in its centre with a glafs lens, which admits of the infide being feen, at the firft ftroke of the pifton, a ray of vivid brilliant light will be perceived, which is fuddenly difengaged.

This obfervation is owing to accident; it was made for the firft time by a workman in the manufacture of arms of Saint Etienne, who, on difcharging an air gun in which the air was ftrongly compreffed, perceived a very fenfible light at the end of the barrel.

Extract of a Letter from PROFESSOR PROUST *to* J. C. DELAMETHERIE †.

AMONG the memoirs which I have had for a long while to fend to you, you will find one on Berthollet's work, and on what he fays of the hydrates, and alfo the analyfis of a ftone fallen from the atmofphere, at *Sigena*, in Arragon, which was found to be perfectly fimilar to all thofe which have been hitherto analyzed.

Brandy from the carobe tree.

I do not know whether I fhould mention to you a fort of difcovery, which, however trifling it may appear to be, will, neverthelefs be one day of great importance in Spain, particularly in the provinces where the carobe tree *(caroubier)* is cultivated. It is known that the fruit of this tree is only cultivated along all the coaft of the Mediterranean, to feed cattle. However, after a fuitable fermentation it yielded me a *quartillo* (about the bulk of a pound of water) of excellent brandy,

* Bulletin des Sciences, III. p. 209. The latter obfervation of the flafh from an air-gun has been feveral times obferved before: See our Vol. IV. p. 280. It fingularly contrafts the former fact. N.

† From Journal de Phyfique, &c. Meffidor, An. XII.

Dutch

Dutch proof, from five pounds of the dried fruit. This brandy, although it retains a flight odour of the fruit, is by no means difagreeable to the tafte, and I have made liqueurs with it in no refpect inferior to thofe of commerce. I fhall not delay the publication of the details of this difcovery, from which I claim no credit except on account of the influence which it may have on a part of the commerce of the eaftern provinces of Spain.

Method of obtaining pure Cobalt. By TROMSDORFF *.

LET four parts of finely pulverized zaffre be intimately blended with one of very dry nitrate of potafh and half a part of charcoal powder; introduce this mixture by fmall quantities at a time into an ignited crucible, and repeat this procefs for three fucceffive times, by again adding to the detonated refiduary mafs new quantities of nitrate of potafh and charcoal. This being done, mix the mafs with one part of black flux, and expofe the mixture to a red heat for one hour. When cooled, feparate the metallic cobalt, pulverife it, blend it together with three times its own weight of nitrate of potafh, and detonate this mixture as directed before. The iron which was prefent will thus became highly oxidized, and the arfenic acidified, and combined with the potafh. To feparate the latter, pulverife the mafs, treat it repeatedly in water, and feparate the infoluble part by the filtre. The arfeniate of potafh being thus got rid of, digeft the refidue in nitric acid; the cobalt will now be diffolved, and the highly oxidized iron remain untouched. Evaporate the folution to drynefs, rediffolve it in nitric acid, re-filtre the folution, in cafe fome oxide of iron fhould have efcaped unfeparated, decompofe the folution of nitrate of cobalt by potafh, wafh the precipitate, and reduce it by heat.

Pure cobalt.

Method of coating Copper with Platina. By STRAUSS.

Mr. Straufs, who has made a number of good experiments on platina, has fucceeded in applying this metal to defend the face of copper. The folution of platina was precipitated by

Coating copper with platina.

* Gehlen's Journal of Chemiftry, Vol. IV, p. 6, 117.

muriate

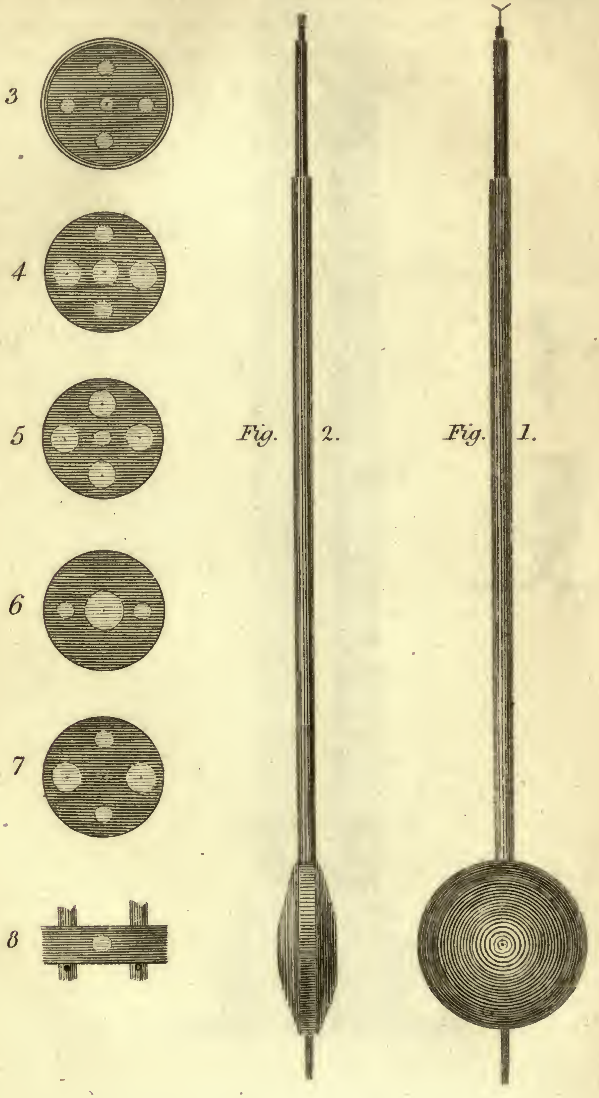
muriate of ammonia, then waſhed and dried, and expoſed to a graduated red heat for half an hour in a covered crucible. The product was a grey coagulated powder; conſiſting of metallic platina in a ſtate of extreme diviſion. One part of this powder with three parts of mercury did not combine by half an hour's trituration; but upon adding two parts more of mercury and ſlightly heating the mortar, he ſoon obtained a tenacious amalgam, which was rendered very ſoft by the addition of two other parts of the laſt mentioned metal.

A ſmall quantity of this amalgam was rubbed upon a plate of copper, which became perfectly covered. The plate was then ignited, and was found to have retained a coating of platina. In the next place he mixed a little of the amalgam with chalk, ſprinkled the mixture with water, coated the plate of copper a ſecond time, and again ignited it. The coating was now found to be very perfect, and aſſumed a ſhining ſilver colour under the burniſher.

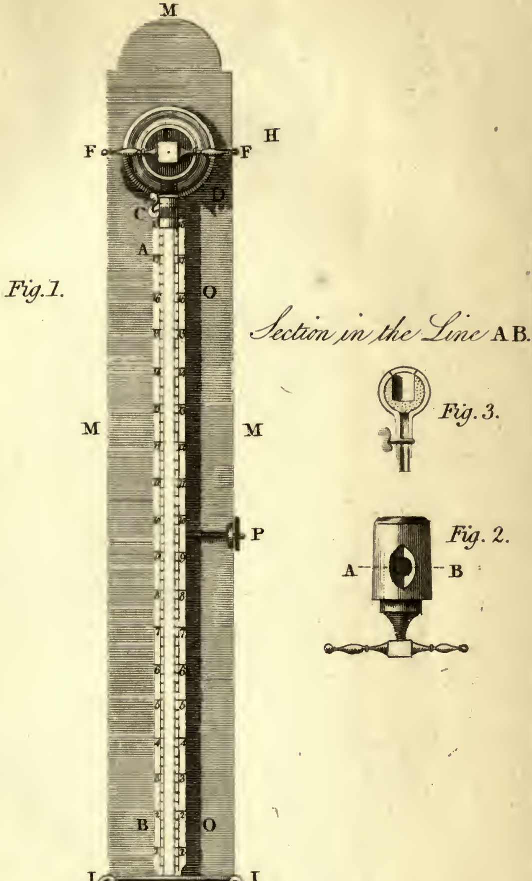
This chemiſt remarks that his application of platina to copper veſſels muſt be ſuperior to that of tin; not only in its reſiſtance to acids and ſaline matters, but in its durability from the greater hardneſs of platina; and he adds that the proceſs here deſcribed, is not more difficult to be effected than the common operation of tinning.

Tromſdorff's Journal, 1803, Vol. II. p. 18.

Tubular Pendulum by Mr. Edward Troughton.



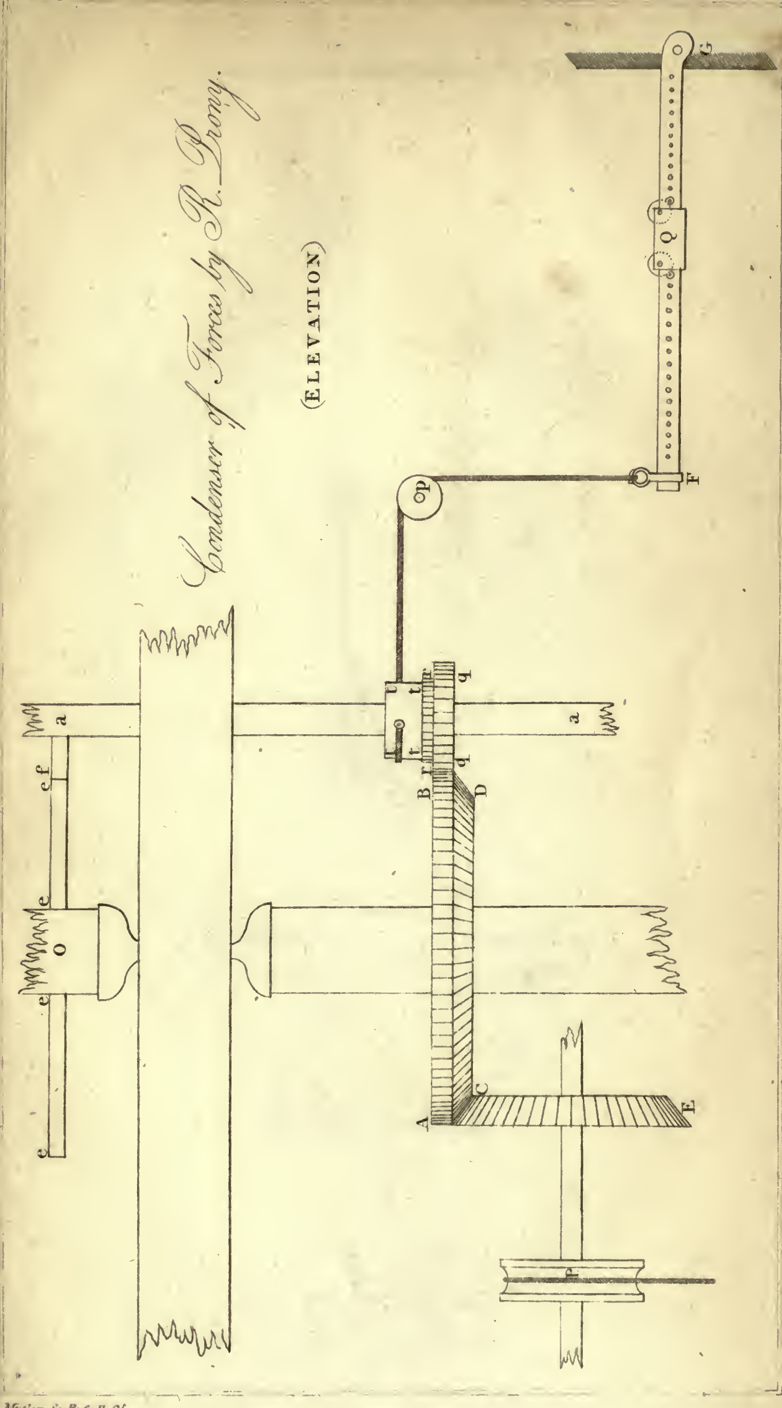
Instrument for measuring the Absorption of the Gases, by Charcoal. by C. L. Morozzo.



Mutlow Sc. Russell Ct.

Condenser of Forces by R. Prony.

(ELEVATION)



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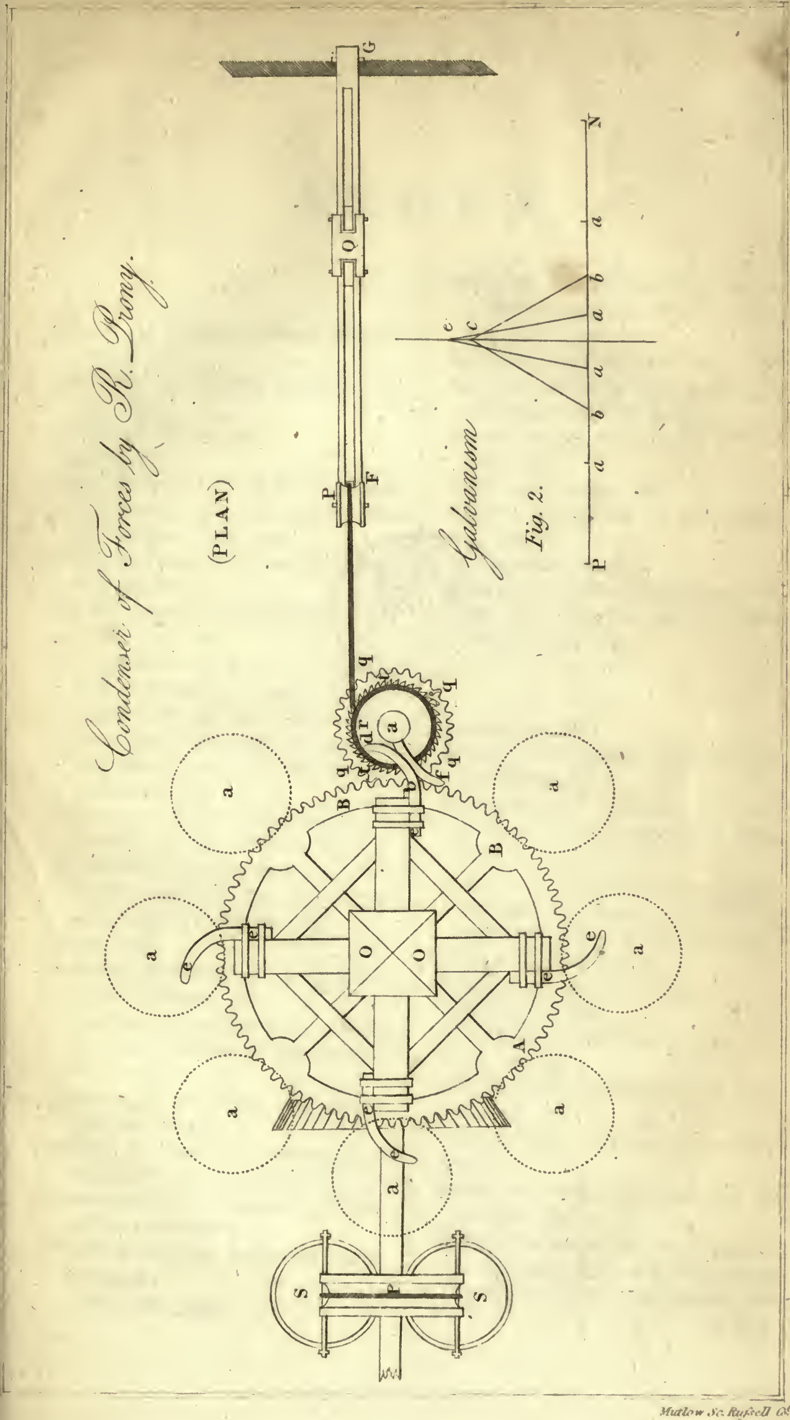

Mutlow Sc. Russell Ct.

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END OF THE NINTH VOLUME.

ERRATUM.

In *Plate* XII. *Fig.* 3, by an omiffion in the defign, the lower ruler has not the fame connection with the frame as the upper. It muft be joined by prolonging the rod from the left hand angle, and adding a rod from thence to the bar upon the ruler; as is feen above.

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